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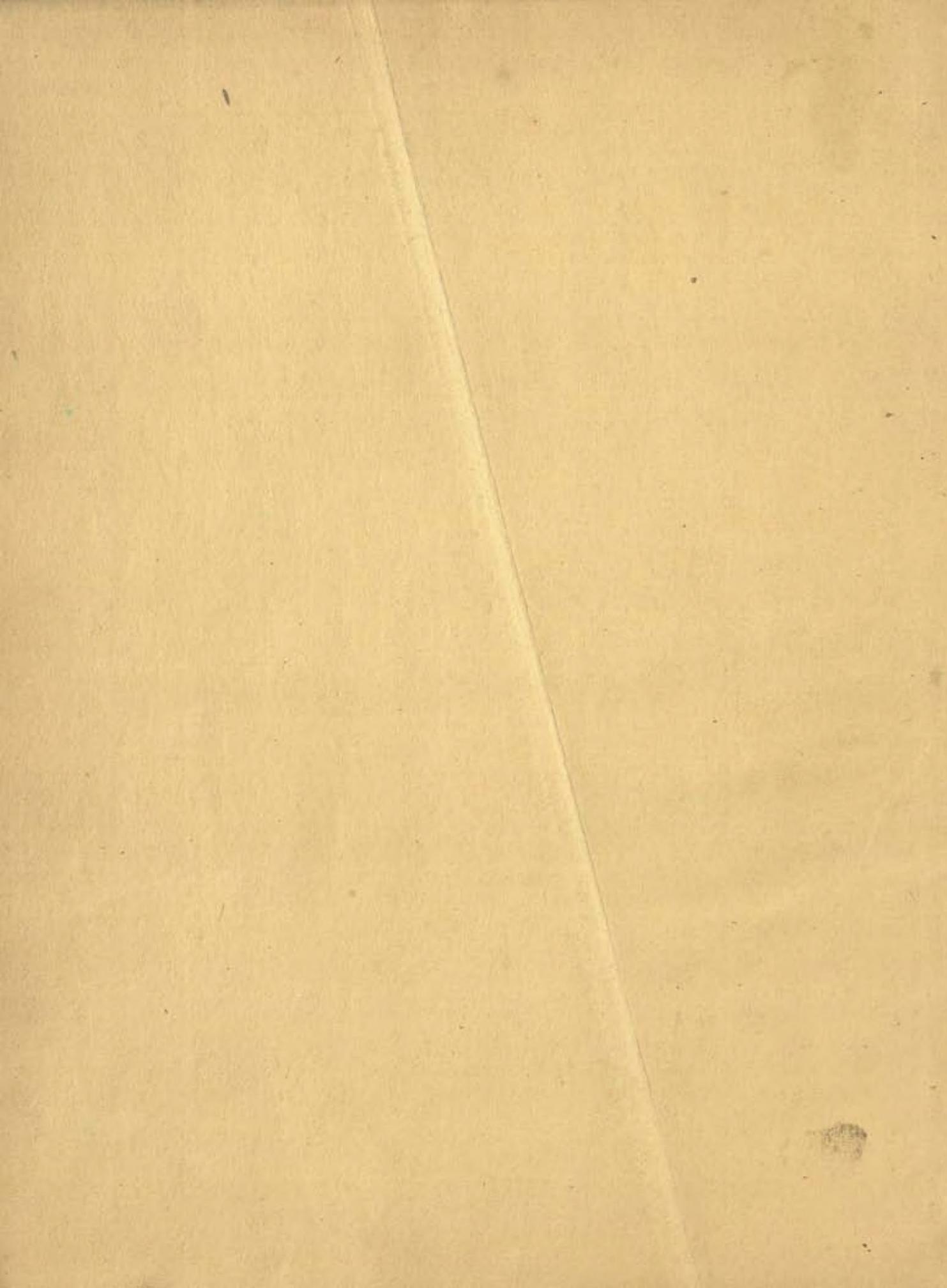
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AD

AS

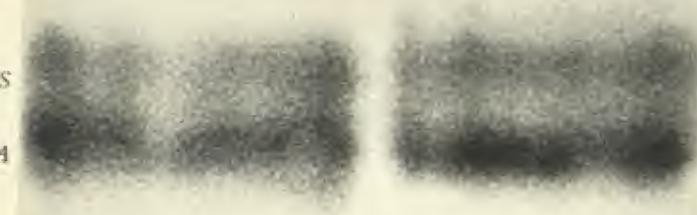
AG

AD

line  
of  
origin

D, S

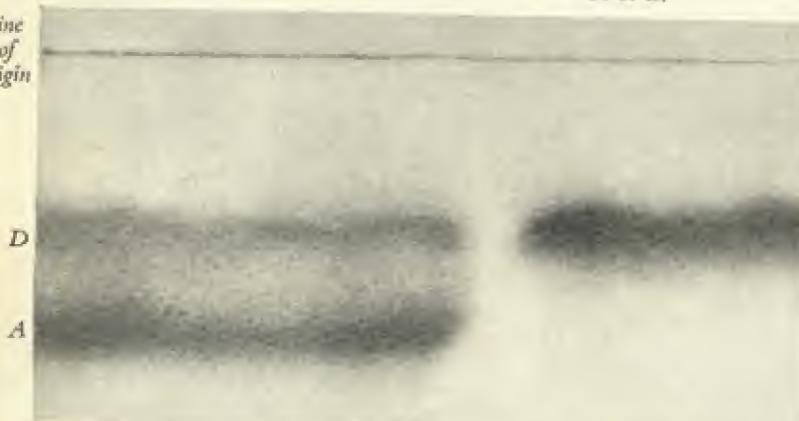
A



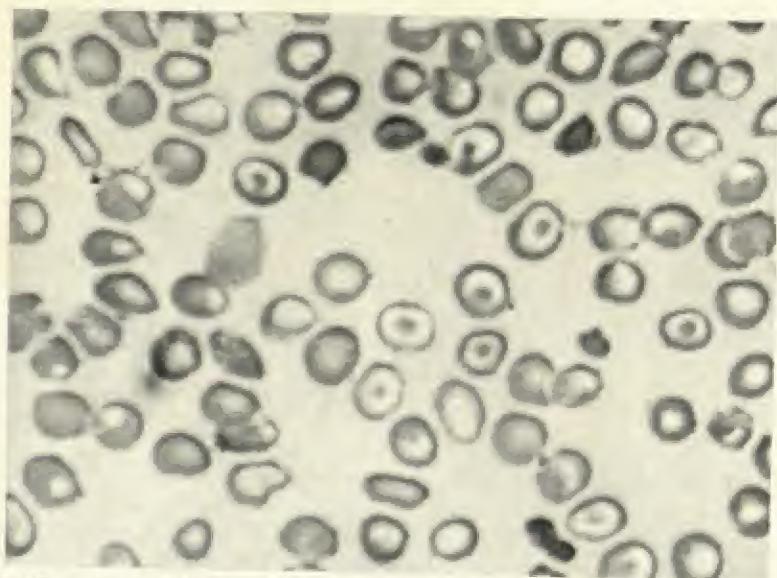
(a) Comparison of AD and AS by paper electrophoresis. Hanging strip technique at pH 8.6. For details see Lehmann and Smith, 1954. The haemoglobin solutions were prepared by washing and packing the red cells, lysing by freezing and extracting with water and toluene followed by centrifuging to remove the stroma.

AD

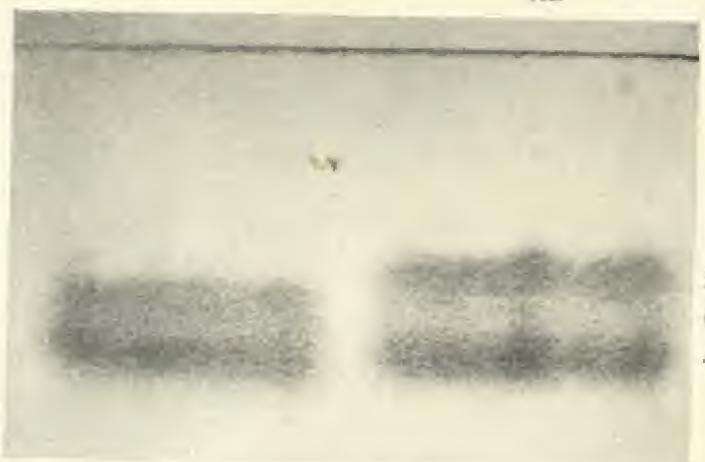
G. S. B.

line  
of  
origin

(c) Comparison of AD and the haemoglobin of G. S. B. (DD).



(e) Red cells of G. S. B. stained after Leishman ( $\times 1000$ ). The area chosen shows numerous target cells.



(b) Comparison of AD and AG by paper electrophoresis.



(d) Crystals of hæmoglobin D ( $\times 65$ ).

# THE FINDING OF HÆMOGLOBIN D DISEASE IN A SIKH\*

by

LT. COL. G. W. G. BIRD, I.A.M.C.

Blood Transfusion Department, Armed Forces Medical College, Poona, India

and

DR. H. LEHMANN

St. Bartholomew's Hospital, London

I Whereas the *S*, *C* and *E* variants of normal adult hæmoglobin (*A*) can be assumed to be present in many millions of people, hæmoglobins *G*, *H*, *I* and *J* have so far been seen in single families or even instances only. Hæmoglobin *D*, though still considered rare, has, however, been found on at least four, and possibly six, different occasions. It was discovered in a 'white' North American family of Irish, English and American-Indian descent (Itano, 1951; Sturgeon, Itano and Bergren, 1955), and was then seen again in a British family with Spanish and Austrian connexions (White and Beaven, 1954). It has been reported to be present in an Algerian Musulman family (Cabannes, Sendra and Dalaut, 1955), in a Mulatto family where it was inherited from an English mother (Stewart and MacIver, 1956), and in a Turkish family (Aksoy and Lehmann, 1956). On an examination of 109 Sikh soldiers for blood groups and for hæmoglobin variants, one was discovered to be heterozygous for the genes responsible for hæmoglobin *A* and *D* (Bird, Lehmann and Mourant, 1955). We have therefore investigated a further 62 unrelated Sikh soldiers for abnormal hæmoglobins, and another instance of hæmoglobin *D* was found. This time it was present without any other hæmoglobin, and the phenotype *DD* was observed for the first time.

## Hæmoglobin D

The hæmoglobin of G. S. B., a 25-year-old Sikh soldier, was examined on three different occasions. On paper electrophoresis, both by the hanging-strip method and by the horizontal method with the filter paper enclosed between glass plates, only one component was seen. Its speed of migration was that of hæmoglobin *S* or *D*. The electrophoretic properties of *S* and *D* are indistinguishable by any known procedure, but *D* differs from *S* by the higher solubility of the reduced ferro-hæmoglobin (Itano, 1951), and by not causing the sickling phenomenon when it is reduced intra-cellularly. In the present case the cells were reduced with 2 per cent. sodium meta-bisulphite, and no sickling occurred; and whereas in Itano's solubility test (Itano, 1953) the ferro-hæmoglobins of *SD* heterozygotes and *SS* homozygotes show a solubility below 5 g. per litre of Itano's 2·24 M phosphate buffer at 25° C., the ferro-hæmoglobin of G. S. B. showed complete and ready solubility at this concentration. The only other hæmoglobins with similar electrophoretic mobility are foetal hæmoglobin (*F*), and hæmoglobin *G*. *F* is recognized by its resistance to denaturation by alkali. The hæmoglobin

of G. S. B. was fully denatured in the standard one-minute test for alkali resistance of Singer, Chernoff and Singer (1951). Hæmoglobin *G* differs only slightly in its electrophoretic properties from *S* and *D*, and like *D* it does not cause the sickling phenomenon. Hence it is necessary to compare a hæmoglobin thought to be *D* very closely with known *S*, *D* and *G* controls. On paper electrophoresis specimens must be tested under exactly parallel conditions, and on the same paper strip. Thus particularly in the cases reported from Algiers an examination of the published electrophoretic patterns suggests that further work will be required to exclude the possibility that the new hæmoglobin found was not in fact hæmoglobin *G*. Plate A, *a-c*, shows comparisons of *AD* samples with *AS* and *AG*, and with the hæmoglobin of G. S. B. respectively. In addition to the comparisons reproduced here, the hæmoglobin of G. S. B. was run in parallel with *SS*, *EE*, *GG*, *AS*, *AE* and *A+F* samples. All tests fully confirmed that this hæmoglobin was indistinguishable from *S* and distinct from the other variants.

When the reduced ferro-hæmoglobin of *G* was dissolved in 2·24 M phosphate buffer at a concentration of 5 g. per litre at 25° C., and the solution cooled subsequently at 4° C., crystals formed readily (Plate Ad).

## Blood Picture

When G. S. B. was himself examined following the discovery of hæmoglobin *D* in his blood, it was found that his blood picture showed unusual features.

G. S. B., *aet.* 25, unmarried, was a Sikh from the Jhelum district of what is now West Pakistan. His present home is Srinagar, Kashmir. He was seen in Poona, and as he had no relatives in South India it has not been possible up to now to examine his family. He remembered no previous illnesses, and 'had always been hale and hearty.' He appeared to be fit, and 'was doing a good day's work like anyone else.'

On physical examination nothing abnormal was noted, and in particular the liver and the spleen were not palpable. X-ray pictures of the skull, femur and metacarpal bones showed no abnormality. Findings on his blood were as follows (normal range for adult men in brackets):

Hæmoglobin: 12·8 g. per 100 ml. (14-18)

Red Cells: 7,100,000 per cubic mm. (4,600,000-6,200,000)

Reticulocytes: 0·7 per cent. (0-2)

Packed Cell Volume: 45 ml. per 100 ml. (40-54)

Mean Corpuscular Volume: 63 cubic  $\mu$ . (82-93)

Mean Corpuscular Hæmoglobin: 18  $\gamma\gamma$ . (27-31)

\* With Plate A

Mean Corpuscular Hæmoglobin Concentr.: 28 per cent. (32-36)  
 Mean Corpuscular Diameter: 7.6 μ. (7.2-7.8)  
 Mean Average Thickness: 1.4 μ. (1.7-2.5)  
 Osmotic Fragility: haemolysis begins at 0.3 per cent. NaCl, and is complete at 0.2 per cent. (begins at 0.5 per cent. and is complete at 0.2 per cent.)  
 Serum Bilirubin: 0.3 mg. per 100 ml. (<0.8).

As is illustrated by these measurements there was no evidence of undue haemolysis. The red cells were of normal diameter, but very thin and hence their volume was reduced; in addition they were not fully saturated with hæmoglobin. Thus the mean corpuscular hæmoglobin was low. However, the hæmoglobin level of the blood was brought to normal or near-normal by an increase of the red-cell concentration. Plate Ae shows the appearance of the blood smear. The cells looked hypochromic, there was some anisocytosis, and target cells were numerous. The red-cell picture resembles that seen in hæmoglobin E disease. Had an analysis of the hæmoglobin not been performed, the decreased fragility, and the finding of numerous target cells might have led to the diagnosis of a compensated thalassæmia, possibly thalassæmia minor. Poikilocytes were, however, absent and the anisocytosis was moderate; hence the findings would not have been quite typical for thalassæmia. It is noteworthy that very similar observations have been reported from the Punjab, and it might be worthwhile to re-examine cases of thalassæmia reported from North-West India for abnormal hæmoglobins.

The finding of two individuals with hæmoglobin D among 171 unrelated Sikhs—one of them presumably a homozygote—suggests that this hæmoglobin may be less rare than had been expected from its absence in surveys of many thousands of Africans, Europeans and North Americans. We hope to continue the survey of Sikhs, and to extend it to other related Indian populations, particularly to Punjabi Hindus from whom the Sikhs are derived.

#### Note

We should like to thank Dr. A. E. Mourant for his interest in this investigation and Captain C. S. V. Subramaniam for his help in examining blood specimens in Poona.

A further series of 108 non-related Sikhs and 13 Punjabi Hindus is being examined. Although the work is not yet fully completed, it seems already that four more instances of AD have come to light, three in Sikhs and one in a Hindu.

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## UNILINEAL DESCENT GROUPS IN THE NEW GUINEA HIGHLANDS

by

R. F. SALISBURY

*Harvard University*

2 Unilineal descent groups<sup>1</sup> have been found all over the world in societies 'in the middle range of relatively homogeneous, pre-capitalistic economies in which there is some degree of technological sophistication, and value is attached to rights in durable property,'<sup>2</sup> as Fortes points out in his summary of the characteristics of these groups. As he says, he deals mainly with African data, but his conclusions are generally applicable to all such groups. I wish to show how the unilineal descent groups occurring in the New Guinea Highlands conform, in the main, with the characteristics of the African groups, but also show some striking differences. These differences, by showing how it is possible for unilineal descent groups to dispense with long genealogies, throw some light on a problem pointed out by Fortes<sup>3</sup>—namely 'the limits of genealogical depth in lineage structure.'

The peoples whom I wish to discuss are the Mbowanib, on whom there is published material, and the Siane, a

group of recently contacted tribes possessing a relatively uniform culture and a common language, and with whom I worked. Both societies have similar economic levels—a garden agriculture involving artificial reafforestation and a calculation on 15 years of regeneration under trees, following each period of cultivation; a complex system of ceremonial trade; a technology based on highly evolved polished stone axes; a population density approaching 100 per square mile.

I studied the Siane for a year in 1952-1953.<sup>4</sup> They occupy an area of about 180 square miles of precipitous mountains in the New Guinea Highlands just west of Goroka, and they number some 15,000 people. They live in villages of about 250 inhabitants, which are strung out along the razor-backed minor ridges of the mountains at about 6,500 feet. Typically each village is occupied by one patrilocal, patrilineal exogamous clan, and the wives who have married in from other clans. Formally the social

organization is of the segmentary kind, with no overall political authority. Tribes are the largest social grouping recognized in the native terminology, and there are 16 in the area. Each one occupies a well defined territory. They vary in size between 400 and 1,500 individuals, and are composed of between two and nine localized clans. They are kin groups in the sense that it is possible (though not usual) to call any member of one's own tribe 'brother.' They have a common name but are otherwise only loosely unified groups which are never seen acting as a unit, or functioning for the settlement of disputes, unless the tribe is small. If the tribe is small, it coincides with the widest exogamous group, the *nenta wenena*,<sup>5</sup> or 'close people,' within which marriage is forbidden as is warfare to kill a member of the group. This group of two or three clans acts as a unit in the biggest religious ceremony of the area—the triennial *Yafu Koiya* or 'Pig Feast'—when each exogamous group honours all its ancestors. Like the tribe, this group is a kin group, since all members claim 'brotherhood,' but it too is only loosely unified and it has no powers to enforce the settlement of disputes, despite the statement that 'warfare is prohibited' within it.

The clan, of about 250 individuals living in one village, is the largest commonly effective unit for the organization of work and ceremonies; it has a proper name; it is always an exogamic unit; within it kinship terms or personal names (as against tribal or clan names) are always used; it combines to fight as a unit and is the unit for exacting revenge in the blood feud. Within the clan there are also smaller effective units comprising all the men—about 40—who occupy one men's house. Since sons normally take up residence in the men's house of their father, these units tend to be patrilineal descent groups in addition to being residence groups. Since the houses of wives of members of each men's house are grouped around the men's house, it is also a residence grouping for the women and young children. In addition, this unit is held together by the fact that boys tend to find their three or four close friends within it, and the boys remain together all their lives as a group of inseparable age mates.

One can also discern a grouping smaller than the men's house, and which roughly parallels the African lineage, though the natives do not distinguish it or have a name for it. It comprises three or four nuclear families *plus* their married sons, who always use kinship terms for one another, and who are united in having one senior member in each generation, for whom the members of all families use the term *yanefo*—'my oldest brother.' It is this grouping that an informant thinks of first when he is asked 'Who are your brothers?', and only secondarily does he think of the kinship term 'brother' as referring to members of the men's house, clan or tribe. This is the grouping within which most rights to property are inherited, but unlike the 'true' lineage, it has no traceable common ancestor. In view of its size, and for convenience, I shall refer to it as a 'lineage.' In short, the system of segmentation in Siane society has five levels—about four lineages combine to form a men's house group, two or three men's house groups form a clan, two or three clans form

the exogamous group, and from two to nine clans form a tribe.

To cite all the ways in which these descent groups conform with the characteristics of unilineal descent groups, as outlined by Fortes, would be a mere repetition of points brought out in the classic African studies. Suffice it to say that they are groups in which membership is by virtue of descent, and all members stand in real or fictitious kin relationships with all other members. They are corporate groups, as has been shown already in their corporate obligations in the blood feud. They are groups which, as far as an outsider is concerned, are internally undifferentiated. For example, if Fanti, a boy of Tene clan, Ramfau tribe, wishes to marry Koi, a girl of Winyo clan, Emenyo tribe, then the marriage is phrased as taking place between Ramfau tribe and a sister of Emenyo tribe; the principals will not appear, and the actual ceremony is performed by the 'big men' of the two clans involved, each one representing his tribe. This example also shows how groups have complementary relationships with other units of the same order of size; this is also attested in disputes where men's house group will line up against men's house group, or they will combine to fight as one clan against another clan. Groups are also conceived of as having a permanent continuing existence, even though the particular personnel at any one moment may change.

For the lineage, this continuity takes the form of there being certain kinship 'statuses' that are kept perpetually filled, through a system of promotion on the death of any member. Thus, in each generation of the lineage there is a *yarafo* or eldest brother, and he has a quasi-parental role towards his *kunarafo* or younger brothers. For the children of the lineage, all of the paternal generation are *merafo* or fathers, but the eldest brother is *homu merafo* or 'first father,' the head of the lineage, and its representative in relationships with other lineages. The next oldest 'younger brother' is *merafo ainke* or 'next father,' while all other brothers are *merafo airo* or 'other fathers.' Succession to these kinship statuses is mainly by seniority, so that if a member of the lineage dies (or 'retires') the next senior member moves up into his status. Thus if a 'first father' dies, the next oldest brother becomes *yarafo* to his own generation, and *homu merafo* to all the children's generation. The oldest 'other father' becomes 'second father,' and so on. When all the parental generation have died, the *yarafo* of the next generation succeeds to the representative role of 'first father.' He has already been in a quasi-parental role *vis-à-vis* his younger brothers, so there is little discontinuity in his behaviour; also he is likely to be almost the same age as the youngest of the paternal generation so that there is no sudden break in the lineage headship—no youth succeeding an old man. In fact, the division into generations can become quite arbitrary—if a man in the parental generation is younger than the oldest brother of the children's generation, he will be absorbed into the children's generation, calling his classificatory son, *yanefo*—'my oldest brother.' With this system of succession, the lineage is always composed of a 'first father' and several successors, and in each generation

there is an 'eldest brother.' These are the structural bones of a lineage.

For the clan or men's house, there is no such system of kinship statuses which are kept perpetually filled. The only differentiated status—that of 'big man' or *we namfa*—who represents the clan or men's house in dealings with other units of the same size—is an achieved status and one to which any individual can aspire.<sup>6</sup> The way in which the continuity of these groups is symbolized will be described later; here it must be stated that the continuity is *not* expressed in terms of a genealogy—perpetuity is not conceptualized in a genealogy as it is in African groups.

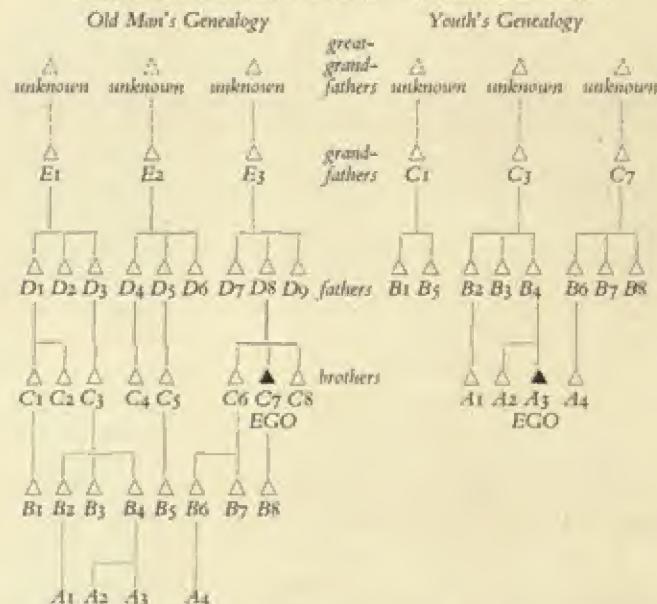
Despite my detailed questioning, no informant could trace a genealogy going further back than to his grandfather, or in a few cases, to his great-grandfather. Everyone agreed that it was many generations ago—more than ten as they would indicate by holding up both hands—when the original tribal ancestor emerged from the ground. They agreed that there had been only one ancestor for each tribe, but no one could trace descent back to that ancestor. There were no genealogies showing how the various clans, men's house, and lineages were related to each other, and so providing a 'conceptualization of the existing structure viewed as continuing through time and therefore projected back as pseudo-history.'<sup>7</sup> The system of segmentation was related as having been created by the original ancestor—usually by non-procreative means—and to have remained so ever since.

The short genealogies of Siane are used to give a picture of the existing structure of the smallest descent groups—the lineages—as was shown to me when I found how the perceptions of the lineage genealogy differed from generation to generation.

Thus when I obtained a genealogy for one lineage, from a boy of about 18, it showed that there were eight men in the parental generation, both living and dead, some with children and some without. These eight men he gave as the children of three men of the grand-parental generation. However, when I tried to get the name of the father or fathers of these men, the boy could not answer me. The three men were 'just brothers'—they were the only brothers, they were the children of different fathers, but the names of the fathers were unknown. In other words, the boy knew nothing about the great-grand-parental generation, except that it was composed of three different 'fathers' who were not genealogically linked, but who were members of the same lineage. Later I obtained a genealogy from a man of the grand-parental generation of the same lineage and aged about 55. He gave the same picture for the parental generation—his 'sons'—with eight names. But for his own generation he named eight men, in place of the three the boy had named. The ones the boy had named had not only *not* been the sole members of the grand-parental generation but had not even fathered all the men of the parental generation. The boy's insistence that they were the sole members of that generation does indicate that they were the only three significant figures for him. Going further back, the old man gave me nine names for the generation of his father—all of which had been completely

unknown to the boy—and three names for his grandfather's generation. I asked him the same questions about these three men as I had asked the boy about his grandfathers, and I got the same answers—they were just brothers, there were no other brothers, they had different fathers. A comparison of the two forms of the lineage genealogy given in Table I shows how they both represent a similar conception of the lineage organization.

TABLE I. DIFFERENT PERCEPTIONS OF ONE LINEAGE GENEALOGY



Names are indicated by letter, and males only are shown. Note the 'reorganization' in the youth's genealogy: B5 is given as the son of C1; B6 and 7 are given as sons of C7.

I obtained similar pictures for other lineage genealogies. The significant feature is that individuals know that the lineage is the same size as it was in their father's day—about eight men—and that it stays about the same size in their children's generation. But irrespective of what generation they themselves belong to, they think of the lineage as having been composed of three individuals in their grandfather's day. These three men, they know, had different fathers, and so the lineage is projected back, through unknown and unnamed generations with a presumed composition of three individuals.

It is only a short step to seeing those three individuals as 'first,' 'second' and an 'other father,' and the genealogy, as it is perceived, then appears as the charter (to use Malinowski's term) for the structure of the living members of the lineage. The genealogy differentiates by generation and by seniority, and shows how three statuses in each generation of the lineage have always existed.

I have already stated that there is no charter for the segmentation and structure of larger descent groups in terms of a genealogy, as occurs in the classic African pyramidal segmentary structures. Why this can be so is partly to be found in another big difference between Siane descent groups and African ones. Despite all my inquiries to find cases, I could find almost no evidence that the groups were 'in the process of continuous further segmentation at any

given time<sup>8</sup> as is characteristic of African groups. As has been shown, the myths describe the segmentation of the clans as having been present since legendary times. No one seemed interested in 'explaining' the division of clans into men's house groups, and the answer I repeatedly obtained was 'Well, they couldn't all live in one men's house.' There is likewise no pattern of sibling groups splitting, and one section going off to found a new settlement and to start a new descent group.<sup>9</sup> Instead, the sibling group of lineage brothers is held together by the constant expectation of brother succeeding brother, and generation succeeding generation. Nor do the rare cases of uxorilocal residence result in new descent groups springing from the immigrant man. Two husbands out of 69 were so living in my own village, and always the migrant was spoken of as being a member of his wife's lineage, since he was living there. His children are considered the 'brothers' of their cross-cousins, with whom they are living. By the time they have children in their turn, the name of the immigrant grandfather will have been forgotten, and the children will be members in their own right of the lineage of their grandmother. In other words, the seceding member of another village does not become the founder of a new descent group; instead he and his children become incorporated in existing lineages. This can be done simply, because of the short genealogies and the speedy forgetting of remote ancestors.

In short, the existing pattern of segmentation is thought of as having always been present in Siane, and the existing relations between segments are seen as unchanging relationships. Potential splits are patched up by incorporating seceding members into other lineages, and within three generations the genealogies have been 'clipped, patched and telescoped'<sup>10</sup> to mask the changes under an appearance of stability. Genealogies thus give an appearance of stability to what is in reality transient. In Siane they conceptualize the structure, continuity and group unity of the lineage; they are not used to represent these aspects of the constitution of larger groups. How can this be?

I believe, on the basis of the comparison which I am making, that the answer is that there is no need for such abstract (or fictitious) methods of conceptualization in Siane, since the relationships between all groupings are more concretely expressed in the spatial distribution of these groups. All descent groups are also residence groups, which means that lineages live next to one another in a common men's house area; men's houses cluster in a village; the constituent clans of an exogamous group are neighbours, and in this way set themselves off from the other clans of the same tribe with whom they intermarry; yet the tribe as a whole is also a residence group. The few cases of migrant clans stress this general rule of relationships being in accord with residence—they are fitted into the kinship structure of the tribe on whose land they have been given permission to settle, and call all tribal members 'brother.' Yet where they have been included into an exogamous group within the tribe, it is their nearest neighbours whom they do not marry, and whom they feel to be their 'nearest brothers.'

The continuity of the larger groups is also conceptualized in ties with the land. Religiously this is expressed in the relationship between the ancestral spirits and the land—a theme which is stressed in the first-fruits ceremonies for each garden, in the fertility rituals before both the first-fruits ceremonies (*ka maso* or Yam-Taro ceremonies) and the triennial *Yafö Koiya* or Pig Feast, in the legends in which the original ancestor emerged from a hole in the tribal land, and in the idea of ancestral souls remaining in the area where they lived and needing placation and exorcism in funerals.

The charter for the unity of the group and the expression of the interdependence of parts of the wider groups is also given by the religious organization. Thus each lineage is associated with one particular pair of sacred flutes, which represents, in general, ancestral spirits, and it also has one particular geometrical design which represents the souls of dead members of the lineage. In addition there is one special musical horn associated with each exogamic group. At the *Yafö Koiya*—the only time when the exogamic group operates as a whole—all the various flutes in the group must be present, as must the souls of all the dead, as represented by the various designs painted on boards called *gerua* and carried in the dance by members of the lineages concerned. In this way all lineages are necessary to the completeness of the exogamic group. It is only on the day preceding the killing of the pigs that the big horn is produced—the sign that the largest effective grouping is corporately involved—and this is phrased by the natives as 'The Old Man has emerged.' This is the closest I could get to any explicit statement of the central mystery of the religious system, a mystery which is clearly the expression of group unity and common descent.

The crucial question then is why in some societies genealogies perform these functions of conceptualizing the structure of the group or expressing its unity and continuity, while in other societies these functions are performed by religious organization and by spatial distribution. In order to suggest an answer, I propose to compare the Siane with the Mbowamb,<sup>11</sup> who live some 100 miles to the west of the Siane in the Mt. Hagen region of the New Guinea Highlands. They have a formal social structure which is similar to the Siane structure, already described, in its levels of grouping. The Mbowamb, however, have genealogies of a depth of about eight or nine generations, and all levels of group segmentation correspond with some split in the genealogy at some generation level. They fit almost exactly the African paradigm. Local groups are centred round an individual 'big man,' who maintains his position of headship by ceremonial trading, and by his genealogical connexion with his ancestors, of which he keeps tangible evidence in the form of relics. The other members of the society keep their loyalty to him because of their kin relationships, but segmentation of these local groups into smaller groups is a common pattern. In that case, the migrant section keeps up a kin tie with the original settlement, even though it may move to a completely new area. One could then say that the correlates of the organization of society in terms of

genealogies are formal authority positions and the presence of fission. The conditions where this syndrome occurs, and the conditions where the Siane syndrome occurs, are suggested by another contrast between Siane and Mbowamb.

The Mbowamb have a history of conquest by invaders, a moving population and a population which seems generally to have been increasing<sup>12</sup>; the Siane have had a population both stable in size and stable in location. The suggestion is then that long genealogies and the appropriate social organization occur under conditions of increasing population and population mobility, while stability of population size and location means that genealogies can be dispensed with.

The population stability of Siane is indicated by the demographic data. Thus, my figures for births, still-births and miscarriages over the last 50 years<sup>13</sup> show that 45 women now past the age of child-bearing, or dead during the child-bearing period, had a total of 129 pregnancies resulting in 96 children who reached maturity—44 of them female. The estimated net reproduction rate would be about unity, or what is needed for a stable size of population. This stability is confirmed, not only by the stability of lineage size, as indicated in the genealogies, but also by the typical family size. Most families are of two or three children, and this pattern is supported culturally by the wide spacing of pregnancies through the religious post-partum sex tabu, by the late commencement of cohabitation by the men in view of the sanctions of the men's house cult, and by the fact that a bride price is considered 'repaid' by the wife's giving birth to two children; if she runs away or refuses to bear more after producing two, no bride price is returnable to her brothers.

Stability of location has already been indicated by the close association in myth and religion between a clan, its ancestors, and the land it inhabits. This association is recognized by other clans to the extent that warfare for territorial gain was unknown. Even when a clan was defeated in battle and went into exile, the victors would allow the exiles to return to their land when a settlement was eventually made. More concrete evidence is provided by deserted village sites. Regularly there is a shift of village site within the clan area about every 30 years, and each clan can point out at least three of these sites within its area, indicating that clan areas have not changed in 100 years. The sites also appear to be about the same size as the present villages, supporting the idea of stable population size. The migrations that there have been are known, and they all happened in legendary times. Out of 36 clans whose history I obtained only 14 do not claim to have been created by the original ancestor who emerged from the ground on which the clans now live.

In short, in stable societies, land and religion can be used to give adequate stress to the continuity and unity of the groups, and to conceptualize the group structure. In societies that are moving or expanding, the most stable thing about the groups is the people who make them up; the most noteworthy thing about each group is the leader, who is the individual remembered in the genealogies.

To such groups a genealogy gives an impression of stability.

There is not the space to quote extensively from the literature for groups outside of New Guinea, but the situation of stable non-expanding groups with their descent-group continuity and unity being phrased in terms of the residence of the group, and of the religious organization, is obviously present in the North American Pueblos.<sup>14</sup> Type cases of moving, expanding populations with long genealogies are frequent in Africa among such peoples as Tiv and Nuer<sup>15</sup>—to mention only two—although the other type of stable static population and short genealogies may be found, for example among the Plateau Tonga.<sup>16</sup> Among these peoples the group structure is conceptualized in terms of offices having religious sanction in the cult of rain shrines, associated with the land.<sup>17</sup> With them might be contrasted the Zulu, whose expansion in the last two centuries from being a stable society in similar conditions has been matched by a remembering of the genealogies of the group leaders during that period. In Polynesia too, the longest genealogies occur in those places to which the migrants had to travel furthest, and where the population growth from single boatloads has been the most rapid—in places like New Zealand and Hawaii. In places like Samoa where the population, although large and well segmented, was relatively stable and stationary, the genealogies are shorter, and we find a greater emphasis on ties to land, and on ceremonial offices attached to groups residing on particular pieces of land.<sup>18</sup>

In summary, the unilineal descent groups of the Siane of New Guinea, although basically similar to those described by Fortes, suggest the following hypothesis. The hypothesis is that group unity and continuity are conceptualized in concrete terms, such as identification of the group with land, and a complex religious organization, when the group population has been stable and static. Genealogies are used for such conceptualizing when the group has been mobile and expanding, since genealogies give an appearance of stability. The hypothesis is supported when it is considered that even a 'stable' society uses genealogies to give an appearance of stability to its most changing element—the internal structure of the lineages. Genealogies can and do perform all the functions outlined by Fortes—conceptualizing group continuity and structure, expressing group segmentation in pseudo-historical form—but these functions can be performed by other institutions. This paper is an attempt to specify some of the conditions under which genealogies are used by societies, in preference to alternative methods.

#### Notes

<sup>1</sup> A short version of this paper was read at the meeting of the American Anthropological Association in Boston, November, 1955.

<sup>2</sup> M. Fortes, 'The Structure of Unilineal Descent Groups,' *Amer. Anthropol.*, Vol. LV (1953), No. 1, pp. 17–42. All quotations in this paper are from this article.

<sup>3</sup> Fortes, *op. cit.*, p. 31.

<sup>4</sup> Fieldwork was undertaken as a Research Scholar of the Australian National University, whose assistance and support is hereby gratefully acknowledged.

<sup>5</sup> The spelling of native terms follows the phonemic alphabet for Siane described in R. F. Salisbury, *Grammar and Wordlist of the Siane Language, Eastern Highlands of New Guinea*, Mikro-Bibliotheka Anthropos Monographs, Fribourg, 1956. A short ethnographical summary enlarging on points in this paper may be found there.

<sup>6</sup> Although the importance of wealth and ceremonial knowledge in achieving upward mobility does tend to favour the sons of 'big men.'

<sup>7</sup> Fortes, *op. cit.*, p. 27.

<sup>8</sup> Fortes, *op. cit.*, p. 31.

<sup>9</sup> I obtained only one myth with an implication of sibling hostility and migration following a dispute between brothers. In observation of family interaction I saw no sibling rivalry, while in response to a projective test using a situation of sibling rivalry over a woman as a stimulus, natives would alter the stimulus to one of brother-brother solidarity against the intruding female.

<sup>10</sup> Fortes, *op. cit.*, p. 32.

<sup>11</sup> G. F. Vicedom and H. Tischner, *Die Mbowamb: Die Kultur der Hagenberg Stämme im östlichen Zentral-Neuguinea*, Vol. II, Hamburg (Friedrichsen, de Gruyter), 1943 (particularly Chap. 1).

<sup>12</sup> Vicedom and Tischner, *op. cit.*, pp. 2f. The figures given on p. 23 show that 36·3 per cent. of Mbowamb females are younger than 15, while only 29·7 per cent. of Siane females are. This indicates a higher birth rate for the Mbowamb.

<sup>13</sup> The lack of concealment of still-births, etc., and the ease with which women could and would give dates, relative to the age of living individuals, for these unsuccessful pregnancies suggest that the figures are more or less complete.

<sup>14</sup> F. Eggan, *Social Organization of the Western Pueblos*, Chicago (U.P.), 1950.

<sup>15</sup> E. E. Evans-Pritchard, 'The Nuer of the Southern Sudan,' in *African Political Systems* (M. Fortes and E. E. Evans-Pritchard, eds.), London (O.U.P.), 1940, p. 280; Laura and Paul Bohannan, *The Tiv of Central Nigeria*, London (Int. Afr. Inst.), 1953.

<sup>16</sup> E. Colson, 'The Plateau Tonga of Northern Rhodesia' in *Seven Tribes of British Central Africa* (E. Colson and M. Gluckman, eds.), London (O.U.P.), 1951.

<sup>17</sup> Fortes, *op. cit.*, p. 37.

<sup>18</sup> Margaret Mead, *Social Organization of Manus*, B. P. Bishop Mus. Bull. 76, Honolulu, 1930, pp. 134f.

## OBITUARY

### R. M. Dawkins: 1871-1955

*Adapted from the obituary published in the Oxford Magazine, Vol. LXXIII, No. 23, June, 1955, by courtesy of the Editor*

Richard MacGillivray Dawkins, who died suddenly at Oxford on 4 May, 1955, was a figure who could ill be spared from a university in which scholarship becomes every year more departmental and statistical; his devotion to every branch of art and letters and learning (though not to music), combined with an appetite for friendship and laughter, seemed to belong to a past generation. He was born in 1871, spent his childhood at Stoke Gabriel in Devonshire, and began his schooling at Totnes Grammar School. Then he was sent to Marlborough, where his short sight and inability for games made him unhappy, although he was befriended by at least one of the masters, who inspired him with a lifelong passion for botany. On leaving Marlborough he was sent to study engineering in London, at King's College, and was subsequently apprenticed to a well known firm of electrical engineers at Colchester. On the death of his father, a retired Rear-Admiral, he gave up electrical engineering and, after a year or two of desultory reading and travel, went up to Emmanuel College, Cambridge, as a thirty-year-old undergraduate, studied philology and classics under Giles, Adam and Postgate, and was elected a Fellow in 1904. He had already been admitted to the British School of Archaeology at Athens in 1902, and in 1906 he was appointed Director of the School, where he remained for eight years, his sister keeping house for him. 'The New Director,' notes the Annual Report, 'has a high reputation as archeologist, as philologist, and as explorer. So unremitting has been his interest . . . that he has hardly been in England since four years ago he was first associated with the School. . . . His protracted travels have given him an exceptional knowledge both of modern Greek life and of ancient survivals in the less known parts of the Levant.' (It may here be noted that in the same year, 1906, he contributed to the *Journal of Hellenic Studies*, Vol. XXVI, an article of considerable importance to anthropologists on 'The Carnival in Thrace'.)

As Director he excavated at Sparta (the sanctuary of Artemis Orthia), at Phylakopi in Melos, and at the Apollo temple on Sikinos, besides visiting and drawing many other sites. In 1914, his last year as Director, he began by the customary visit to Crete, and at the end of the session visited Trebizond, where he remained till the middle of August after the outbreak of war. He was already

correcting proofs of his first book, *Modern Greek in Asia Minor* (C.U.P., 1916), which, besides 150 pages of dialect folk tales, and a study of the dialects of Silli, Pharasa, and Cappadocia, with 100 pages of Greek and Turkish glossaries, also contains a chapter on the subject-matter by his lifelong friend (Sir) W. R. Halliday. In 1916 Dawkins took a commission in the R.N.V.R. His services as interpreter all round the coasts of Crete have been noted by Vice-Admiral Sir Gilbert Stephenson (*Times*, 18 May, 1955); and Sir Compton Mackenzie, in one of his reminiscences, has described the startling appearance on deck in an Aegean gale of two new officers—Dawkins with his red hair, and J. L. Myres with his stiff neck and black Assyrian beard. Meanwhile Dawkins had inherited from his mother's cousin John Doyle, of All Souls, an estate in North Wales, where he was to make his home for the rest of his life—when he was not at Oxford or travelling in Greece or Italy.

In 1920 he was elected to the newly established Bywater and Sotheby Chair of Byzantine and Modern Greek Language and Literature; when he first came to Oxford he was adopted by All Souls, but soon moved to Exeter College, with which the Chair later came to be permanently associated. His retirement in 1939 made very little difference to his life, as he was made an Honorary Fellow of the College and continued to occupy his old rooms and to work with his usual intensity. He always taught less by formal instruction or lecture than by example and conversation, and of course by his writings; and a thick flight of detached papers preceded and followed all his books. No undergraduate who wanted to learn could fail to be educated by a Professor who gave such amusing parties; who seemed to have read the whole of European literature; and had the unique advantage of being able to contemplate and enjoy the whole of Greek literature, Ancient, Mediæval and Modern, including the ancient as well as the modern dialects.

*The Cypriot Chronicle of Makhairas* was not published till 1932, an immense and successful labour, involving the collation and conflation of two different versions of the same chronicle of the Lusignan Kings, with a study of the Franco-Cypriot dialect. *The Monks of Athos*, issued four years later by a London publisher, is the nearest thing he ever did to an ordinary travel book—a blend of description, tradition and history, coloured by his rare gift of writing about simple people without laughing at them. He had collected a mass of notes for a second edition of this book, and it is to be hoped that good use will be made of them.

On the outbreak of war in 1939 he worked for a time with the Chatham House survey of the foreign press at Balliol, but soon gave all his attention to preparing *45 Stories from the Dodekanese* (C.U.P., 1950), a rather too massive quarto in double columns. These had been collected by a local antiquary more than fifty years ago for W. H. D. Rouse; and are here edited with every luxury that could be desired by the fastidious dialectologist; and with an introduction in which Dawkins examines problems of the meaning and classification of folk-tales which were to occupy him for the rest of his life. In 1952 the Clarendon Press brought out *Modern Greek Folk Tales*, followed early in 1955 by *More Greek Folk Tales*, two books which provide, in English translation, a corpus of the best folk tales which can be regarded as specifically Greek. Dialectology had now given place to the psychology of folk lore, and an illustrated selection of the same stories, in a popular series 'for children and young people,' is also in preparation.

Dawkins must have had a wonderfully happy life until 1948, when he fell down some steps in a dark corridor at a friend's house and broke his thigh; and, after a long period of pain and painful

hope, had to make up his mind to walk for the rest of his life only with the aid of two sticks and a steel support strapped to the un-mended leg. He moved to rooms smaller and more accessible, but still bright with books, ikons, Rhodian pottery, and paintings by Ethelbert White, John Nash, William Roberts and Roger Fry. His walks were now confined to the adjacent gardens; and he went on working most of the day and far into the night, banging away at a typewriter which he had contrived to make as illegible as the most feverish handwriting. As before, he was never too busy to entertain an old friend, famous or infamous, or to be entertained by an original undergraduate; never too busy to begin or to continue a learned or an outrageously frivolous conversation. He was himself a brilliant and breathless and insatiable talker, modest and unselfish in conversation, never showing the jealousy he sometimes felt, and nearly always attributing his best jokes to somebody else. Schoolboy or sage, he remained a figure any university and any country could be proud of; and after his remarkable personality and appearance have been forgotten, his works will be a proper memorial.

J. M.

## ROYAL ANTHROPOLOGICAL INSTITUTE PROCEEDINGS

**Banyang Secret Societies.** By M. J. Ruel, M.A., B.Litt., Institute of Social Anthropology, University of Oxford. Summary of a communication to the Institute, 20 October, 1955

**4** The Banyang, with a home population of little more than 18,000 people, live in forest country in the extreme upper basin of the Cross River, their territory now forming part of the Mamfe Division of the Southern Cameroons. Secret societies have always played an important part in their social life. One of the most important aspects of the societies is their relationship to and expression of political authority, particularly in their use as a governmental sanction for the maintenance of order in the community. At the present time the most widespread society with the strongest political or governmental powers in Ngbe, a society which is here taken to illustrate the way in which they may assume political functions.

Ngbe, which is found generally in the Cross River area, originally entered Banyang country from their western neighbours, the Ejagham. The term *Ngbe* (pronounced variably as *Rgbe* or *Dkps*) is the Ejagham word for 'leopard.' The symbol of the leopard which is central to the society is nevertheless expressed less explicitly amongst the Banyang than it is amongst the Ejagham, and the society is occasionally described as *Nya-Ngbe* (or *Nya-Nkpe*) 'the animal of *Ngbe*.' In both cases the names *Ngbe* and *Nya-Ngbe* refer to the society as an association and to the 'leopard' or 'animal' which is said to be kept by the society and which makes itself known to outsiders by its 'voice,' a loud, booming noise which is heard on special occasions, when the society meets formally or one of its chiefs dies. The ferocity of this 'animal' is emphasized. It is apparent that it is not, in fact, a real animal, but its exact nature and the means by which the 'voice' is produced are kept secret, being known to only a few initiates.

Spread over the whole country, *Ngbe* consists of many independent lodges, each having its own organization based on one common model. In becoming a member of one lodge one obtains membership of the total society and has the right to enter any of its other lodges, wherever they may be found. Membership of the society is open to any male member of the village and in fact the majority of its men usually are members of *Ngbe*. The purchase of a separate lodge may be made by any man who has

sufficient wealth and following. Since position within the society is related to wealth and social prestige the leading members of a village are normally found to occupy the senior and more influential positions. One village usually contains more than one lodge of the society, besides societies other than *Ngbe*, and these lodges, although acting with a common purpose in society matters, will usually represent sectional interests in the village.

A key feature of the society is its division into a series of progressive stages, each of which must be entered by the payment of a special fee. These stages are not all of the same type: some relate to sub-groups of the main society, each of which has its own internal organization and meets as an exclusive group; others to separate status positions or ranks in the main society or its sub-groups; finally there are certain marked stages in the obtaining of esoteric knowledge of society emblems and rites. Entry into each stage is progressive for the individual and is the means by which he may gradually improve his position within the total society. The money and goods spent in fees will thus bring him certain privileges, will enable him to enter and participate in the more exclusive groups of the society and will give him the right to take his own share in the later divisions of fines and fees within these groups. This system of 'stages' or of groups which have a growing exclusiveness tends towards the maintenance of the total exclusiveness of the society as a whole, its institutionalized and corporate unity providing the basis for both its authority and strength. This unity is maintained and expressed in a number of ways: in the 'secrecy' laws which prevent society activities from being discussed or repeated outside; in the esoteric knowledge concerning society objects held by its members; in the symbols (such as the 'leopard' or 'animal' mentioned above) which represent the society as a corporate group.

One of the most important aspects of the society lies in the means which it provides for common recreation or entertainment, its members meeting primarily for the purposes of communal eating, drinking, singing, discussion, etc. At the same time the society may be invoked as a sanction in the promulgation of laws or the protection of property or to enforce a judgment made by the senior men of the village. A *Ngbe* sign, placed on private property, safeguards its owner's rights, a trespasser who is caught being accountable to the society and not simply to the owner of

the property. Laws which are announced publicly by the beating of the *Ngbe* drum have in the same way the backing of the society as a corporate group. The ultimate sanction of authority which may be used when an individual will not otherwise accept any judgment against him is the making of a *Ngbe* law which debars him from all social intercourse with other members of the community. All members of the village, whether they are members of *Ngbe* or not, should keep the laws made in this way. Although promulgated through *Ngbe*, these laws are nevertheless derived from the village authority itself, represented in its leading members (its chief, sectional heads, senior men, etc.). The leading members of the village are themselves members of *Ngbe* and in their action for the common good may use the society as the means of formalizing the authority which is otherwise represented in their own persons.

The political function of *Ngbe*, as of other secret societies, should be regarded according to the nature of Banyang political authority. The first and most important condition of this authority is that it is corporate. No individual, whether chief or senior man, wields authority in his own name. It is always the function of the group, acting corporately, any judgment given being always made in the name of the group, the 'town' or village community, the *erok*. *Ngbe*, when used as a sanction for authority, provides this authority with a formalized and thus a stronger means of expression. In practice the authority of *Ngbe* rests on the fact that it does include those men who are, in fact, the leading members of the village. They, in their turn, may have recourse to the formal sanction of *Ngbe* which expresses their own corporateness as a group. As Banyang say, '*Ngbe* is the town.' It represents thus the corporate authority of the 'town,' the village community, in a form which derives its strength from the institutionalized unity of the society.

**On Segmentary Lineage Systems.** By Dr. M. G. Smith.  
Summary of a Cull Bequest Prize Essay communicated to the  
Institute, 5 January, 1956

Analysis and understanding of segmentary lineage systems is inseparably bound up with the definition of those systems. Segmentary lineages are groups recruited on the principle of unilineal descent and organized at all levels in terms of an order of balanced segments which are formally distinguished in the lineage genealogy.

The theory of segmentary lineage systems as developed by Evans-Pritchard and his followers distinguishes sharply between the lineage principle and other kinship principles on the ground of the political primacy of lineage affiliation in segmentary societies. In fact, however, the definitions of political relations and action which provide the foundation for this distinction between kinship and lineage are unsatisfactory in various ways.

Any distinction between segmentary and non-segmentary societies cannot be reduced solely to differences of political systems, since all political systems are inherently segmentary in form. Moreover, together with administrative organizations, political systems are a component of governmental systems, and government is by definition an aspect of all societies. Political action is thus an aspect of behaviour and not a special type. As such it occurs within groups as well as between them. Action is political which focuses on the determination of policy as that is defined by the group concerned. Action is administrative which implements this political decision or is concerned with the routine management and coordination of group affairs. Administrative organization is hierarchic in character while political action is latently or overtly competitive and has a segmentary function and form. Units constituted by a certain type of balance in the distribution of these administrative and political functions from

their base to their apex display the segmentary form and character such as has been observed in lineage societies. They have this segmentary form and character by reason of the cross-cutting combination of both hierarchic and segmentary principles, the former administrative and the latter political, about which they are constituted. To the extent that this particular balanced distribution of political and administrative functions among the parts of any system shows a preponderance of political or administrative interest, the structure of the series departs from the familiar model characteristic of segmentary lineages. Hence the analysis of segmentary lineages as a political form *sui generis* is self-defeating.

The segmentary lineage system can only be understood within the context of a general comparative study of governmental systems of all types. Such an analysis requires a theory of government general enough to embrace all known and probable forms but specific enough to provide verifiable propositions about the operations of any particular system. In such a study governmental myths, of which lineage genealogies, corporateness and indeed the concept of lineage itself are simple illustrations, must be recognized for what they are, images and myths about the reality, not the reality itself. To approach and understand this reality, abstract analytic categories distinguishing relations between administrative and political action are essential.

Misunderstanding of political systems, and definition of segmentary lineages in terms of such systems, has gone hand in hand with an unfortunate dichotomy between lineage and kinship; yet most lineages, being exogamous units, are defined significantly by marriage, and marriage cannot be treated separately from the kinship system. Non-exogamous lineages themselves reveal the operation of kinship principles in the arrangement of marriages.

These inadequacies of segmentary theory are most clearly revealed by current treatments of lineage form and formation, which are discussed at some length in the essay.

**The Kinship System of the Tallensi: A Revaluation.** By  
P. M. Worsley, M.A., Ph.D. Summary of a Cull Bequest Prize  
Essay communicated to the Institute, 5 January, 1956

Professor Fortes's studies of the social structure and, in particular, the kinship system of the Tallensi people of the Gold Coast constitute a major landmark in recent social anthropology.

He concludes that the 'essence' of Tale kinship lies in 'its function as the primary mechanism through which the basic moral axioms of [the society] are translated into the give and take of social life.' This appears to me an inadequate conclusion to such a rich and many-sided work, and expresses an underestimation of the critical significance of economic factors in Tale life.

Fortes's own material amply shows that in Tale agriculture the home farm near the compound is the key economic asset. The joint family forms an economic unit under the leadership of the father, and it is this productive system which primarily conditions such features of Tale life as the continuity and solidarity of the lineage, respect for and dependence on the father, the ancestral cult, and so on.

However, land is limited; those who cannot be provided for are obliged to seek land or wage labour elsewhere. Moreover, Tale society is economically differentiated. In order to advance their own interests sons must often flout the authority of the heads of households. Fission in the lineage is profoundly affected by such considerations, and is not merely the fruit of the conflict of structural 'principles' of matrilineality *versus* patrilineality.

Professor Fortes emphasizes the 'irreducibility' of three systems in the total Tale society: the economic, the kinship and the ritual. These appear to me as not strictly commensurable, in so far as kinship, in a society of this kind, is a general idiom in which

multiplex relations are expressed, and in which general moral axioms are couched. This special importance of kinship derives not from the 'neutrality' of economic factors, but from the special significance of these very factors. The specific relations—domestic, political and other—expressed in the idiom of kinship and lineage must therefore be separated out.

## SHORTER NOTES

### III Session of the Pan African Congress on Prehistory held in Livingstone in July, 1955. By M. C. Burkitt

**7** Briefly, the facts are these. The third Pan African Congress took place and was attended by about 130 delegates and members—mainly the former—from some 26 nations. The Congress was supported by a Northern Rhodesian Government grant of £2,000 towards excursion and publication expenses. The Southern Rhodesian Government subscribed £500 and various business concerns gave £800. The Belgian Government provided a sleeping car train with a restaurant car attached free of charge for a week for those taking part in the excursion in the Katanga Province of the Belgian Congo.

Three excursions were arranged. The first took place before the Congress proper started when a number of important sites in Southern Rhodesia were visited. These included Khami, Zimbabwe and several painted rock-shelters in the Matopos as well as Stone Age implement sites. About 50 delegates and members took part in this excursion, which was 'bear-led' by Mr. Roger Summers, the Curator of the Bulawayo Museum. It need hardly be said that lengthy discussions took place at Zimbabwe in regard to its age and cultural connexions! These were particularly interesting in view of the fact that Dr. Caton-Thompson took part in the excursion. A 'sundowner' was given to the party by the Mayor of Bulawayo. Some of us who were present could not but regret that the late Dr. Neville Jones, who did so much to elucidate the prehistory of Southern Rhodesia, had not survived to be one of the party.

On our return to Livingstone the Congress proper started with an opening ceremony graced by the presence of Lord Llewellyn, the Governor-General of the Federation. The sessions were divided into three main sections: I. Quaternary Geology, II. Human Paleontology, and III. Prehistoric Archaeology. Important papers were read in each section. The problem of the so-called pebble-tool cultures was much to the fore in a joint symposium of all three sections. A number of specimens were put on show so that members could handle them. For myself, I came to the reluctant conclusion that it is difficult to believe that these alleged tools could not have been the products of natural fracture, and this seemed to be even clearer when some of us examined a large site in the Congo between Kasenga and Elizabethville, where gravels occur full of these Darnsden-like objects. Various members of the Congress collected what they thought were undoubtedly human artefacts, but frequently they seemed unwilling to accept each other's choices! Everybody admitted that man existed during these early times, but until some unrolled Kafuan examples are found in a more or less closed site the incontrovertible proof that they are indeed artefacts would seem to be wanting. No specimens that we were shown showed three directions of blows in the fracturing (the criterion for human workmanship demanded in connexion with the eoliths of East Anglia) and less than this can, of course, be produced by natural forces. Moreover as the specimens we saw were rolled, sometimes very heavily, one could not even swear that they were contemporary with the gravels in which they occurred.

A subject which created great interest in the Prehistoric

In more recent studies by Professors Fortes and Forde, inquiry into the conditions necessary for the formation of unilineal descent groups has been initiated, with full emphasis on the ecological and economic factors which I have mentioned. Important comparative studies remain to be carried out along this line of research.

Archaeology section was the rock-shelter art. Mr. J. Walton of Basutoland put forward the claim that, far from being of vast antiquity, most of the paintings in the Union of South Africa were comparatively modern, some of them as late as the early seventeenth century. He pointed out that some of the earliest examples in Basutoland were representations of Bantu ceremonies, and he suggested that they were the result of contact between the native Bushmen and the earliest Bantu arrivals. This would mean that the rock-shelter paintings further north, though obviously considerably earlier, would still be of no very great antiquity and would at any rate date, say, to within the Christian era. Personally, I have long felt that it is in general difficult to accept a vast antiquity for paintings in rock-shelters because they would hardly have survived the various considerable atmospheric changes that have taken place. Near Kasama in Northern Rhodesia I noticed some paintings on exfoliated granite surfaces where weathering had not yet smoothed out the rough rock surface resulting from the exfoliation. The cave paintings in France and Spain have survived because the 'climate' in the caves has not changed. A new gallery near the entrance at Bédeilhac which had been completely blocked by a little landslide was discovered during the war. It was full of paintings, but these disappeared within six months as soon as atmospheric changes penetrated into the little gallery. Much of the Nachikufan art in parts of Northern Rhodesia could possibly have been early Bantu. Many happy arguments about this took place at a number of sites visited, Dr. Desmond Clark taking the view that they were a good deal older.

Particularly interesting was a paper given by Bernard Fagg of Nigeria on a rock-shelter in his district where exfoliated slabs of granite ring when hit so that a sort of *Glockenspiel* effect can be produced when a number of people play on them. In the rock-shelter itself there are paintings and when the rocks were 'played' dancers appeared as they would for weddings, etc. A combination of cinema and gramophone effects brought the whole scene vividly before us, and it was interesting to note that the dancing girls had whitened their faces. Can we recall in this connexion the famous 'White Lady' painted in a rock-shelter in South-West Africa?

Among many other interesting papers given to Section I, several were devoted to the problems of the Kalahari sands, while Section II was occupied with two main subjects. There was a symposium on the Australopithecine, and Professor C. Arambourg gave a detailed account of his discoveries at Ternifine in Algeria of two human mandibles associated with an Acheulian industry and mammalian fossils.

During the Congress a number of local excursions took place and archaeological sites and prepared sections near Livingstone and above the gorges of the Zambezi and Maramba rivers were examined.

One fact seemed to emerge very clearly in the course of the Congress, namely that subsequent to the end of Acheulian times the Sahara seems to have become a real barrier to ethnic movement, and the developments of the cultures to the south of it were therefore entirely different from those in the north. Natur-

ally during damper phases the Sahara dwindled, but, so far as it goes, the evidence seems to show that at no time could it have been easily traversed after this date. Anyone who has flown over the great desert realizes at once what a barrier it is. At various periods east-west movements were possible and we get influences from the Upper Nile on the Atlantic coast: but whereas in Acheulian times the African elephant seems to have been able to penetrate to the Atlas Mountains and at least two vast prehistoric lakes can be demonstrated in the heart of the Western Sahara, in later times the separation of north from south was complete. The only exception seems to have been in Kenya, where some slight influences from the north apparently penetrated.

The use of northern names for Central and South African cultures would thus seem to be out of place, and a general nomenclature more particularly applicable to Central and South Africa would be an advantage. A committee of the Congress considered this matter and devised a scheme. The early Old Stone Age, as I have said, is common to both provinces. The Chelleo-Acheulian of the northern province can be directly correlated with the Stellenbosch of South Africa and the *coup-de-poing* industries of Central and East Africa. From then follows, it was suggested, the First Intermediate period when occurred the various Sangoan industries, which differed a great deal among themselves in various geographical regions although they all seem to belong to forest cultures; and, contemporary with the Sangoan, the Faure-smith cultures of the open country. After the First Intermediate there follows the Middle Stone Age. This is a complex embracing various kinds of Still Bay industries, as well as the Petersburg culture of South Africa. The advantage of a generalized term like Middle Stone Age is here clearly visible since the Still Bay of South Africa while similar to is certainly not the same thing as the Still Bay in Northern Rhodesia. Following on the Middle Stone Age we have the Second Intermediate Period. This includes all that has been classed as Magosian. Once again, the Magosian industries at such a site as Howieson's Poort are not the same thing as the Magosian of Magosi, the type-station at a little water-hole in Uganda. It is of course true that the Magosian of Magosi is a late second intermediate culture while much of the Magosian of South Africa is of rather earlier date. The advantages of a use of the term Second Intermediate would seem to be very obvious. Finally there is the Recent Period, which includes such cultures as the Wilton, Smithfield, Nachikufan, etc. It is very much hoped that this general scheme will be accepted by prehistorians for the areas south of the Sahara, and it does not, of course, rule out in any way the use of local terms so long as it is made clear where these local terms would fit into the general scheme.

The second and very extensive excursion covered almost all of Northern Rhodesia except the western part and the Fort Jameson area. Dr. Desmond Clark led the excursion, which was attended by some 40 members. Many important sites and decorated rock-shelters were visited. Work began almost at once as a number of gravel sections between Livingstone and Lusaka were searched and not a few interesting specimens discovered. Dr. Clark's site at Twin Rivers near Lusaka, which is still in process of excavation, was visited and a number of Bantu engravings in the vicinity explored. Further eastwards, near Serenje, an interesting demon-

stration of native iron-smelting and bark-cloth-making had been organized. The former is a dying trade and nowadays practised only by the older generations. Rock-shelter paintings at Nsalo and Nachikufu were inspected and discussions took place about the alleged Kufic painted inscription there. Near Abercorn Dr. Clark's marvellous early Palaeolithic site close to the Kalambo Falls was visited, where he has exposed *in situ* a number of prolific levels of late Acheulian date succeeded by Sangoan (which seems to be largely an evolution of what came before) with some Middle Stone Age specimens above.

From Abercorn the party turned westwards to Kashiba on the Luapula river, which actually is a tributary of the Congo. Near Kashiba at the Munwa stream a group of interesting rock carvings were visited before the main river was crossed and the Congo excursion started. This was attended by some 30 members and was led by Dr. Mortelmans, Professor of Geology in the University of Brussels. Once again a large number of sites were visited and many interesting things seen and important collections made. The excursion was facilitated by the loan of the train already mentioned, and also by the cooperation of the directors of the various local copper, cobalt and uranium mines. It was particularly interesting to study the newly discovered Kansenian industry, which has been recently published by Dr. Anciaux de Faveaux and to visit Dr. Cabu's splendid museum at Elizabethville.

The Congress with its excursions was an enormous success and was fruitful in every way. The President, Dr. Louis Leakey, although still recovering from a bad attack of bilharzia, was indefatigable, and the Secretary, Dr. Desmond Clark, had taken infinite pains to make everything run smoothly. It was due to his care and skill and to Mrs. Clark and their helpers and to the marvellous hospitality of public bodies and private people in Livingstone and at every place where stops were made that the Congress, as well as the excursions, was so enormously enjoyed by all who took part.

It is hoped to hold the fourth Pan African Congress in three years' time in the Belgian Congo. This is something to look forward to. Naturally the sites visited this year in the Katanga province will not be included in the programme of the fourth Congress, but there is an enormous amount of prehistory still imperfectly known in the vast area covered by the Belgian Congo and we shall all look forward eagerly to having a chance of making up our minds as to the cultures present and their connexions.

#### International Folk Music Council: IX Annual Conference, Germany, July, 1956

**8** The IX Annual Conference of the International Folk Music Council will be held at Trossingen, Württemburg, 25-28 July, and at Stuttgart, 29-31 July.

The following themes have been selected for study and discussion: myth and ritual in folk song and folk dance; the migration of melodies; and folk music (song and dance) in the present day with reference to problems of its survival, revival and adaptation.

Application should be made to Miss Maud Karpeles, Honorary Secretary, International Folk Music Council, 12 Clorane Gardens, London, N.W.3, for further information or to register as a member of the conference.

## REVIEWS

### GENERAL

**Ethno-Musicology. A Study of its nature, its problems, methods and representative personalities.** By Jaap Kunst. The Hague (Nijhoff) (U.K. agents: Batsford), 1955. Pp. 158, bibl. No price

Jaap Kunst's book is opportune. There is a greater readiness than ever before to discuss and accept the forms of expression of

primitive societies, and to allow them a value of their own. In Britain the tendency has been to treat non-European music as a branch of anthropology. Although the Royal Anthropological Institute has set up a sub-committee on ethno-musicology (a term which some may find unattractive but which Kunst prefers to 'comparative

musicology') and the School of Oriental and African Studies of London University has encouraged an interest in exotic music, none of the schools and faculties of music has so far offered facilities for its study. Kunst inherits a different tradition, in which the emphasis is primarily on music and secondarily on anthropology. Nevertheless, his book continually demonstrates the close partnership that musicology must form with the natural and social sciences and with prehistory.

The core of the book is a 'Manual for ethno-musicologists.' It is a revised edition of his *Musicologica* published in 1950, but 'many particulars from non-European sources' have been added 'with the result that the book is no longer so Europe-centric as it was.' The rest of the book (twice as long as the text) comprises a new and most useful bibliography of more than 2,000 items, several indices and 52 illustrations.

Kunst chooses the measurement of pitch for the subject of the first part of the Manual. The choice presumably reflects his great knowledge of Oriental music: if his chief concern had been African music, he might have begun with a discussion of rhythm. Perhaps because he belongs to a generation that has had to work without elaborate techniques, he still prefers the monochord for field work, and does not mention Gurvin's ingenious combination of electric and photographic techniques that records data never obtained before. A little more surprising is his omission of Father Jones's clever but simple device for the study of rhythm in drumming. The bibliography, however, includes the references to both instruments.

The next section of the Manual is devoted to the recording of folk music and to some of the collections that have been made since Fewkes made phonograms of the singing of the Panamquoddy and Zuni Indians in 1889. Folk music, in spite of its conservatism, adjusts continually to change, and it is really almost impossible to distinguish the traditional element from the contemporary unless the traditional has an unequivocal record. The possession of such records gives the modern musician unique opportunities that no serious student can afford to disregard and the encouragement of recording is obviously of great importance. Too many recordings, even by musicians, have been made in a fog of insensibility and ignorance, and it is perhaps too much to hope that the commercial interests that have begun to exploit the market for indigenous music will heed the warnings and advice given on pp. 28-30.

The last part of the Manual indicates the criteria that must be used for the appraisal of non-European music, and discusses their application to the study of racial and cultural relationships. The significance of Hornbostel's Theory of Blown Fifths, once the main argument used to prove contacts of musical cultures, is described with restraint. The reader who wants to go further will be greatly helped by Kunst's careful indications of the most important of the relevant publications. The Manual ends with a discussion of theories of the origin of music, the relationships between vocal and instrumental sound, and the history and classification of sound instruments.

Terminology is the bugbear of ethno-musicology. Vernacular terms, although it can hardly be expected that they will be accepted universally, are used too frequently, and too often without explanation. Even Kunst, although he comments on the ambiguity of a term such as *marimba*, cannot altogether avoid them, but he sometimes adopts the useful compromise of giving the vernacular term, in brackets, after the technical one. There are few other blemishes. Are there, however, any harps in Abyssinia and Eritrea (item P. 405, p. 30)?

The book fulfils the author's aims: to offer a general introduction to the subject, to summarize his teaching, and to encourage self-education. Although the matter is so specialized, and the concentration so great, it is very readable. There is a delightful note on the page on which a report is quoted, from the year 1715, 'that the monkeys of New-Guinea play the flute.' The report, says Kunst gently, 'is not based on the author's own observation (there being no monkeys at all in New Guinea).' The note illustrates the distance that musical criticism has travelled. We may only hope that at some time in the future Kunst will again make a summary of its progress.

K. P. WACHSMANN

**Health, Culture, and Community.** Edited by Benjamin D. Paul. New York (Russell Sage Foundation), 1955. Pp. 482. Price \$5

**IO** This volume consists of a series of case studies of public reactions to health programmes. During the past few years a growing literature has emerged dealing with some of the human factors and problems in the operation of planned programmes of technological change. This is the first study that attempts systematically to report and analyse cases drawn from experiences with health programmes in various parts of the world.

The purpose of the work is to aid public health personnel in understanding how cultural and social factors in the community may influence the extent and nature of the services that they are able to render. Time and again the point is emphasized that in order to carry out health programmes effectively in a community 'it is wise to ascertain the existing habits, how these habits are linked to one another, what functions they perform, and what they mean to those who practice them.' The authors do not attempt to present a series of neatly packaged directives for action, but to clarify for public health personnel the concept of culture and to sensitize them to the possible ramifications it could have for their work.

Sixteen cases are presented of specific health programmes that have operated at the community level in various parts of the world. They are organized under the following rubrics: Re-educating the community, reaction to crises, sex patterns and population problems, effects of social segmentation, vehicles of health administration, and combining service and research. The authors of the various articles consist of social scientists and medical practitioners, with the former predominating. At the beginning of each case the editor makes a statement in which he tries to relate that particular case to others and to tease out some pertinent generalizations of its wider import. Finally, there is brief exposition of the nature of 'culture' that attempts to clarify such concepts as society and culture, race and culture, culture and perception, the patterning of culture, and innovation.

The cases concerned with 're-educating the community' point very dramatically to some of the difficulties of changing those attitudes which are tied up with the larger belief system that governs interpersonal relations and the well-being of the community. A report on a health programme among the Zulus shows that habits pertaining to the consumption of milk were resistant to change because cattle (and all that pertains to them) are intimately related to the veneration of ancestors and the moral codes. Similarly, the people in a small Canadian community were reluctant to change their 'erroneous' ideas of mental health because these ideas provided a rationale which enabled the community to deal with psychiatric deviants in such a manner as to alleviate some of their own anxieties and help to maintain the solidarity of the community. Cases dealing with the experiences encountered in setting up a health clinic in rural Mexico and conducting a health survey in a town in Alabama demonstrated how the work was affected crucially by the existing power structure in these communities. In the former it was the native curers and politicians that interfered with the programme. In the latter the class structure of the community influenced the perception of and participation in the programme. The case presented by Kasper D. Naegele on a mental health project in a suburb of Boston, Mass., makes some interesting comments on some of the problems that arise when social scientists collaborate with clinicians on a research programme.

It appears to this reviewer that the study accomplishes admirably what it sets out to do. The cases are presented and analysed in a lucid fashion without minimizing the complexity of the problems. For medical personnel (and other technicians) working on public health programmes in Western and non-Western communities, this collection of cases should provide some valuable tools for viewing and understanding the social milieu in which they work.

Does the volume make any significant contribution to the social sciences? This question is part of the larger issue of what the role of the social scientist working in an action programme should be. Much of the work reported in the literature (including the study under review) contributes little to the social sciences as such. This

is particularly unfortunate since the cases cited in this volume provide some unique opportunities for making such contributions. For example, among the Zulus, different social practices show varying degrees of resistance to change. It would be interesting to know how the different levels of resistance are related to acculturation phenomena, to changes that are occurring in the social organization, etc. In the discussions of the reaction of the com-

munity members to innovation, it would be important to know more, not only of the modal reaction, but of the personality and social concomitants of the acceptance or rejection of change. Action programmes, as shown by the cases reported in this volume, are a potentially rich field for the gathering and testing of insights in the social sciences.

SEYMOUR PARKER

## ASIA

**Ornamentik der Ob-Ugrier.** Edited by Tynni Vahter. Société Finno-Ougrienne, Trav. ethnograph. IX, Helsinki, 1953. Pp. xxxi, II 216, 200 plates, of which 63 in colour.

The Ob-Ugrian peoples, viz. the Ostyak (in the vernacular Hanti) and the Vogul (Mansi) belong to the Ugrian group of the Finno-Ugrian family. Very few in numbers (18,585 and 5,179 respectively according to the Soviet census of 1926), they live in a very large area on the river Ob and its tributaries. This area is perhaps the most outlying of the whole Eurasian continent, and all cultural influences have reached it very late. Thus most primitive forms of culture have survived there much later than in other parts of Eurasia.

Since the famous explorer M. A. Castrén visited these distant relatives of the Fins in 1845, Finnish scholars (August Ahlqvist in 1877 and 1880, U. T. Sirelius in 1898-1900, K. F. Karjalainen in 1898-1902, Artturi Kannisto in 1901-06) have paid much attention to the linguistic and ethnological study of the Ob-Ugrians. They have brought together very valuable ethnographical collections, especially of textiles and other handicrafts, now preserved in Finnish museums.

The Ob-Ugrian art of ornamentation is the subject of this elaborate study by Miss Vahter. Her interesting investigation (pp. 1-61) of the embroideries on kerchiefs and chemises and especially on long shirtfrocks is the most valuable chapter of the book. The authoress shows here her thorough knowledge of the various techniques of female handicraft. These peoples also used beadwork ornaments. The material came from abroad, and the technique also seems to be loaned from neighbouring tribes. The models used show a great resemblance to those of the embroidery. Further important materials in the arctic craftsmanship are birch bark, hides and wadmal. Especially in the ornaments of these the Ob-Ugrians have used their totem animals as models, as shown by Miss Vahter (pp. 81-130).

The 618 photographs (explained in detail on pp. 135-216), many in beautiful colours, show us the great skill of the industrious hands of the Ob-Ugrian women, their delicate eye in the choice of colours, and their good taste in composing ornaments.

Though there might perhaps be different opinions as to some detail of the explanations given by the authoress, the book is no doubt to be reckoned among the fundamental works about the art of primitive peoples.

PENTTI AALTO

**China's March toward the Tropics.** By Herold J. Wiens. Connecticut (The Shoe String Press, Hamden 17), 1954. Pp. xv, 12 441. Price \$7.50

One aspect of the vast movement of Mongoloid peoples into South-East Asia has been the progressive expansion, through several thousand years of recorded history, of northern Chinese culture and political control, and of her populations, into the area to the south. The book under review represents a comprehensive effort to trace this spread, beginning with the Yang-tzu Valley frontier of the Chou Dynasty, and continuing up to the contemporary pressures on Yunnan, Vietnam, and overseas areas. The author sees the expansion as emanating from the densely populated centre of high culture in the Yellow River plain; as carried by people described as Han-Chinese (not in a chronological sense restricted to the Han dynasty, but as representatives of the orthodox culture exemplified by northern Chinese of Han times); and as leading to the progressive displacement or assimilation of aboriginal inhabitants.

Several quite different, though connected, processes are thus involved in this 'march towards the tropics': the migration of people, the diffusion of culture, the expansion of political control. Though the author makes efforts to clarify these different aspects,

he also at times perpetuates the confusion created by others in this field, and is led to make statements such as, regarding Wu-man culture: 'It also contained alien cultural elements. To these must be ascribed the dark skin colour of the people' (p. 38). Some such lapses result from lengthy paraphrase of previous authors; it is unfortunately often difficult to tell when citations end and the views of the present author reassert themselves. The work is based mainly on Chinese sources, and on previous similar studies in English and German. In view of the broad scope of the problem, other types of evidence are occasionally called for. One might have desired some figures from blood group statistics, or, e.g., relating to population pressure, a slightly more sophisticated use of the sparse, but highly relevant, archaeological evidence.

Through a confusing medley of tribal and geographical names and historical detail, the author maintains a coherent line of argument, leading chapter-wise through the South China geographical environment (I); the tribal cultures of South China (II); the history of their movements (III); their conquest (IV) and progressive displacement (V) by Han-Chinese. The problem of political relations between Han-Chinese and southern peoples receives particular attention in a discussion of tribal uprisings and military colonization (VI), as well as in a chapter on the *t'u-shu* system of administration (VII)—that extraordinary Chinese mixture of indirect rule and feudal enfeoffment of tribal areas. Finally, contemporary ethnic distributions (VIII) and problems (IX) are discussed.

The book is evidently aimed mainly at students of Chinese history and culture, and is clearly a labour of love. The author arrives at no startling conclusions, but gives scholarly and—to a non-Sinologist—convincing body, as well as occasional correctives, to the generalizations of such predecessors as Eberhard and von Eickstedt.

FREDRIK BARTH

**Himalayan Barbary.** By Christoph von Führer-Haimendorf. London (Murray), 1955. Pp. xiv, 241. Price £1 1s.

13 The remote and thickly populated mountain tracts, politically labelled the Assam Himalayas, are of the greatest interest to the cultural anthropologist. It is therefore the more deplorable that they are so little known. Such good and abundant work has been done on the Nagas and other groups of the adjacent Assam-Burma border, that it is tempting to sum up the whole north-east frontier of India in terms of those tribes, and either to ignore the trans-Brahmaputra tracts altogether, or to refer their cultures back in terms of the Nagas. Due very largely to the author of this book, the history is being slowly pieced together of a teeming population of Mongolian tribes, cut off by forest and mountain, in this huge hinterland of the outer, sub-temperate Himalayas. And what wonderfully interesting peoples they are! What a paradise for anthropologists!

While most of the peoples concerned are of the so-called neolithic level of culture, this region includes the semi-nomadic, food-gathering Sulungs, with the simplest of cultures, the more advanced Dafts and Miri tribes, numbering perhaps hundreds of thousands, the Abors of the Siang valley, whose ways are so reminiscent of Nagas (and of whom nothing is recorded), the intensely specialized Apa Tanis, and the civilized Monbas of the Bhutan border.

Professor Haimendorf has concentrated on two of these groups—the Apa Tanis, and the eastern branch of the Dafts. It is doubtful if there is in all Asia a more remarkable tribe than the Apa Tanis. Their population of 20,000 is crammed into only seven villages, clustered round a swampy valley of 20 square miles. There the rich, irrigated soil, worked by a people outstandingly skilful in rice cultivation, started the evolution of a highly organized community

of the greatest possible interest. As the author sums them up (p. 63): 'In the Apa Tani valley we were able to observe the working of an incipient civilization in miniature, a civilization perhaps not unlike those centres of neolithic culture from which—under conditions more favourable than those prevailing in an isolated Himalayan valley—sprang the great prehistoric and protohistoric civilizations of southern Asia . . . collectively they performed a civilizing act such as no other people had performed in those wild and lonely hills ever since the beginning of time. With their industriousness, their passionate sense of order and tidiness, and their budding feeling for beauty, they had created and maintained an oasis of stability within a world of semi-nomadic, improvident tribesmen.'

The intense, regulated manner of life of the Apa Tanis is sharply hit off by their happy-go-lucky, swashbuckling Dafla neighbours, and particularly absorbing are the inter-relationships, the feuds and intrigues between the two tribes, that figure so prominently in this book. In so far as Dr. Haimendorf has recorded his meticulous observations in narrative form, he has given us a travel book. But this is in effect an 'inside' account of the material culture, economics, sociology, religion, and temperament of the peoples concerned. Adding together the intrinsic importance of the subject matter, and the way it is handled, we have an enthralling study that serious anthropologists would do well to read and re-read. For the general reader, the background and ecology of the area is rather taken for granted; and the text is apt to bristle with a bewilderment of proper names of both people and places. The photographs, particularly the portraits, are excellent. Slightly disquieting are the implied strictures on the cast-iron, unsympathetic attitude of on-the-spot officialdom towards these estimable, clever peoples.

CHARLES STONOR

**The Hidden Land.** By Ursula Graham Bower. London (John Murray), 1953. Pp. xx, 244, map, 26 illus. Price 18s.

**14** The hidden land of this exciting travel book is the remarkable valley of the Apa Tani surrounded by, and all but lost among, the forested mountains of the basin of the Subansiri River, a northern tributary of the Brahmaputra. The Apa Tanis were first visited in 1944 by Professor and Mrs. Christoph von Fürt-Haimendorf, who organized and led two exploratory expeditions into this wild and unexplored hinterland. This hazardous but highly successful reconnaissance established friendly contact with the tribal leaders of the hitherto unknown Apa Tani valley and produced a great deal of ethnographical and topographical information. On the basis of the reports prepared by Professor Haimendorf the Government decided to begin the process of consolidation in this frontier region by establishing a permanent Government post deep in tribal territory.

The author's husband was appointed Political Officer of the Subansiri Area, and, accompanied by a platoon of the Assam Rifles to enforce law and order in the immediate area of the outpost, they set off for the hills. This book is a sparkling account of their adventures among the tribes of the Subansiri highlands. Miss Bower writes with great narrative skill, and with an enviable flair for evoking the atmosphere of life in Apa Tani and Dafla villages. The contrast between the peaceful settled Apa Tani and the scattered warlike Dafla is fascinating. The Daflas live in small unstable villages which are 'loose aggregations of long-houses,' practise 'slash-and-burn' cultivation, and are constantly involved in feuds, murders, kidnappings, and the theft of cattle. The twenty thousand or so Apa Tanis live in seven large compact villages within the confines of a single valley some twenty square miles in area. They cultivate rice in elaborate irrigated terraces on the fertile valley floor, and millet on the higher slopes. They have a complex and highly organized social structure based on the division into two endogamous classes, the *mite* or nobility, and the *mura* or serfs; each class being subdivided into exogamous clans. (The description of these classes as exogamous on p. xvi seems a misprint since the author tells us, p. 92, that the 'nobility married almost exclusively within itself'.) The organization is not formally described. Miss Bower's narrative, however, overflows with details of tribal custom, with keenly observed descriptions of behaviour, with accounts of quarrels and feuds and inter-village relations, and with numerous delightful

pen-sketches of individuals: the raw material of anthropology in abundance. It conveys admirably the excitement and the exasperation of fieldwork in this 'hidden land.' It is addressed to a general rather than to a professional audience, and well deserves a success to equal that of the author's earlier *Naga Path*. A full-scale intensive study of the Apa Tani social system would obviously be of the greatest value: may we not have too long to wait before it appears.

COLIN RÖSSER

**Hantu-Hantu: An Account of Ghost Belief in Modern Malaya.**

**15** By J. N. McHugh, illustrated by A. F. Anthony. Singapore (Donald Moore) (U.K. agents: U. of London P.), 1955. Pp. 125. Price 7s. 6d.

Starting off with two brilliant quotations, one from Lewis Carroll and the other from Bertrand Russell, this is a well-written popular book that enlarges our knowledge of Malay folk-lore, though it does not profess to be scientific and indeed is too amateurish to differentiate between (a) the Malay medicine-man and (b) the Malay shaman with his tiger familiar, which is merely regarded as another *hantu*. For the folk-lore here recorded all concerns *hantu*, that is, spirits of nature and the dead and the gods of discarded Hinduism. Actually the author overlooks the last class, having apparently no idea that the Tall (or Great) Spirit and the Spectre Huntsman are in fact Siva, his omnipresence reduced by village wiseacres to ghostly plurality. He seems also unaware that Muslim orthodoxy is content to accept all *hantu*, even Siva, as infidel genies, a concession that has allowed their survival to modern days. At times, in the Frazer tradition, the author goes for his analogies too far afield for the modern anthropologist to follow, and neglects examples nearer to hand. For instance, he omits a Cham parallel for the most gruesome method of acquiring a *pélésit* or nigget. He has caught most Malay *hantu* in his net though he has overlooked the 'Lights of Changkat Asah.' His few attempts at philology are not acceptable, nor would Mr. Wilkinson have claimed to be a great philologist. *Songkei* should be spelt *Sungkai*, and *Carrel* is a misspelling for *Carroll*. The images bodied forth by Mr. Anthony are terrific as they should be.

R. O. WINSTEDT

**Gesellschaftliche Verhältnisse der Ngadha.** By Paul Arndt. S.V.D. Stud. Inst. Anthropos, Vol. VIII. Vienna (Missionsdruckerei St. Gabriel), 1954. Pp. 548, one map. Price 50 Swiss fr.

This book on the Ngadha of south central Flores, Indonesia, contains a really astounding mass of material. Compared with adjoining regions, the Ngadha territory was until recently practically *terra incognita*; but thanks to Father Arndt's previous works on religion and language, and the present one on social organization, the Ngadha are now becoming one of the more adequately described peoples of eastern Indonesia.

The copious information is arranged under the following main headings: Parents and Children; Kinship; Territorial Units; Occupations and Wealth; Law. The entire *corpus* makes up a book which is in the nature of an encyclopaedia of Ngadha life, rather than a work to be read through by whoever wants a rounded presentation of this particular culture: the mass of detail makes it very hard for the reader to see the wood for the trees. As a *Fundgrube*, however, it will be invaluable to anyone interested in Indonesian social organization and related matters.

As in his other works, Arndt presents his data as much as possible in the form he himself obtained it from his informants. The advantage of this procedure is authenticity: one gets facts (at least, facts as seen by the informant) which have not had to pass through the filter of anthropological theory. The disadvantage is that, against the mass of material on some subjects (myth and ritual in particular), there are at times disappointing gaps, even in matters of direct importance for an understanding of social structure. Such an able observer as Arndt could have filled such gaps if he had been more interested in the problems occupying students of Indonesian social organization today.

As in other Indonesian territories, one finds the remarkable wavering between matriliney and patriliney, matrilocality and patrilocality, the marriage locality and descent reckoning being

largely determined by payment or non-payment of the brideprice (pp. 36f.). This interconnection of the two descent principles also seems to appear when one finds that genealogical groups are basically patrilineal, but classification according to rank is matrilineally determined (pp. 321f.). How these two principles interact in practice could have been made clearer than it is now.

Various forms of dualistic opposition between villages, moieties within the village, etc., seem to occur (pp. 19, 122, 126, 381); but again the data from Ngadha would have been a greater aid to an understanding of this very controversial feature in Indonesian societies if the author had not shied away quite so completely from all questions of theory.

Finally, one question which is too often left unanswered is: To what extent does this book describe actual, living practice? Arndt himself says that the information was obtained from 'the best

experts on old customs' (p. 6), and partly refers to 'the days before conversion to Christianity' (Foreword). Of many passages one can hardly be sure whether they refer to the past or to the present. Besides, one often gets facts presented as follows: 'If such-and-such happens, a Ngadha will do such-and-such'—but have such occurrences been observed, or are they out of the ordinary? And is the Ngadha's reaction, such as Arndt (or his informant) describes it, an ideal or an actual type of behaviour? One often gets the feeling that one is reading the script, complete with stage directions, of a gigantic play or pageant, rather than a description of an observed society.

However, it is almost churlish to complain about what the author omits, considering the rich fund of material he has made available. Students of Indonesian social organization will be grateful to Arndt for his latest work. P. E. DE JOSSELIN DE JONG

## EUROPE

**Lascaux and Carnac.** By Glyn Daniel. London (Lutterworth Press), 1955. Pp. 127, 14 plates, 13 figures with end maps. Price 15s.

**17** Not all anthropologists are archaeologists nor all archaeologists connoisseurs of prehistoric cave art or megalithic monuments. Dr. Glyn Daniel has added to his useful accomplishment in popularizing archaeology by producing an alluring and authoritative guide to these two famous and enigmatic centres, accessible—with such assistance—from comfortable holiday resorts, while at the same time touching lightly on modern theories about the people who built the tombs whose stark ruins are so impressive, and the far earlier folk whose art is so fascinating. Here is a geographical and gustatory introduction to recreation spiced with information in the best B.B.C. manner, together with references to original exploratory reports for those who would like to know more at first hand. Are we so sure to-day that civilization always marches forward? Are these cave paintings clues to clearer insight in the past, into the purpose of art in human culture? Are these huge stone skeletal remains of plundered and wrecked mortuary 'chapels' with their graven symbols indications that our European forebears had a religious philosophy rivalling our own in its contradictory conventions? Who were they and when did they live? A guide book such as this unlocks the door to new kinds of 'Who-dun-it' mysteries adapted to the holiday mood—of even the most serious anthropologist.

I am grateful for the 'un-accipital' courtesy of the author's reference to my own deviation from the prescribed megalithic dating line: the evidence for it took long to collect and collate. When it can be published by myself or those who come after me I do not fear contradiction but must content myself now with a hint of the probable outcome in two words—'Remember Zimbabwe.'

V. C. C. COLLUM

**Danish Peasant Culture.** Guides to the Danish National Museum. Copenhagen, 1955. Pp. 92. No price

**18** The scope of this booklet is not as narrow as its subtitle might make one fear. The final part is mainly a catalogue of exhibits in the folk museum, but the largest section is devoted to telling the ordinary visitor something of the life of old Denmark.

First, a short history of farming discusses the different implements used, the importance of livestock at different times and the way they were cared for; while a section on hunting and fishing explains how vital these pursuits were to many farming families, and notes some of the more curious methods employed by them.

The chapter on crafts, besides listing the most important, and local specialties, explains why certain became professional skills, and how they fitted into the division of labour between the sexes. The section on the dwelling house describes the evolution of barn and byre, home and furniture, and gives glimpses of the life for which these things formed the backcloth. Costume is discussed from historical, regional and occupational viewpoints, while the final chapter on feasts discusses the ideas that lay behind the keeping of seasonal festivals and *rîtes de passage*.

The style of the English translation is unfortunately rather heavy, a fault, particularly in a publication intended for a lay audience, but the English visitor should be grateful for the kindness that provides a translation at all.

A more serious criticism is that the illustrations consist solely of photographs, and these mainly of the more decorative objects in the museum; drawings included in the text to illustrate it would have improved the publication considerably. However, within the limits they set, the authors are to be congratulated on a very competent piece of work and one which might well be studied by some museums here.

R. L. HARRIS

## CORRESPONDENCE

**Disguised Languages.** Cf. MAN, 1954, 289; 1955, 111, 112

**19** Sir,—Professor Evans-Pritchard refers to the Zande princes' practice of speaking their language backwards as being rare (MAN, 1954, 289); I beg to suggest that it is by no means rare. Among many of the puberty initiation rites the disguising of one's language either by speaking it backwards or by some similar device is common, as the following examples will show. Thus Cust (*Modern Languages of West Africa*, London, 1883, Vol. II, p. 419) reports: 'A curious practice [among the Swahili] is recorded by Steere called Ki-Nyume: this consists in taking the last syllable from the end of a word and putting it to the beginning. Some individuals are very ready at understanding and speaking this enigmatical

dialect.... Among the elders of the tribe (Pongwe) there is a form of speech called the Ewisia or Dark Sayings, which cannot be understood by the uninitiated, although the council may be held in open assembly. It is formed by changing words in an arbitrary manner, and to no one is the secret confided who has not reached twenty-five years and then only under an oath of secrecy.'

Burton (*A Mission to the King of Dahomey*, London, 1864, Vol. II, p. 152) reported that in the kingdom of Dahomey: 'The neophyte is then removed from his friends to the fetish quarter of the town. There he learns the holy fetish jargon, which is unintelligible to the uninitiated.' Migeod (*Across Equatorial Africa*, London, 1923, p. 165) comments on the reversing of syllables by Negroes as follows:

'The Ngombe [of the Belgian Congo] can talk reversing the syllables of their words when they do not wish to be understood by strangers, which is a custom common in Africa, such distant peoples as the Mende in Sierra Leone and the Swahili of Zanzibar doing it.'

Weeks (*Among Congo Cannibals*, London, 1913, p. 158), speaking of the Boloki in the Congo, says: 'The boys delight in talking a slang language of their own manufacture, which is called *jumu*. They select a syllable, say, *sa*, and insert it between the syllables of the words they use, so *mboka*, a village, becomes *mbo-sa-ka*, vil-sa-lage. They acquire great glibness in this kind of talk, and enjoy the fun that it brings in mystifying others.'

Sibley and Westermann (*Liberia—Old and New*, London, 1930, p. 224) remark about initiation ceremonies in Liberia that: 'Each pupil receives a new name which is to be used henceforth in addressing him: it is considered an insult to call him by the old name which he used as a boy. In some tribes a new language which may only be used within the secret society is learned.'

Northcote Thomas (*Specimens of Languages from Southern Nigeria*, London, 1914, p. 140) made a study of the Ibo practice of each village inventing its own language as it were. He discovered in certain towns specified rules for disguising a language so as to render it intelligible only to initiates. 'The main lines of formation are: (1) inversion of syllables, often with concurrent vowel change; (2) insertion of a syllable or syllables, either in the body of a word or more often as suffix; and (3) occasionally the dropping of a syllable in a reduplicated word or the use of an entirely different word, which is itself reduplicated but has not necessarily either vowels or consonants in common with the original word, etc.' This way of speaking has a special name among the Ibo, namely *akolo*.

While working among the Ibo of Awka division I came across evidence of a secret language in use in Awka town, but I could get no information about it. At Oteri there is a local town slang language in which only those who have taken the trouble to learn it can converse with each other in public without the uninitiated being able to understand a word of it. The Okpala of Oteri stated that each town, and even each age group, devised its own methods of converting Ibo into a secret language, so that though a person might know the *asusu obe* of his own town, or of his own age group, such knowledge was of no assistance in understanding the *asusu obe* spoken in the next town or in another age group. In Onitsha town this form of speech is called *akulu*. From this practice has come the following proverb: *Anulu anyi olu*, 'they spoke *olu* to us.' The explanation is as follows: If two persons are conversing together in a secret language it will be remarked by uninitiates that 'they spoke *olu* to us' or 'it was all Greek to us.'

Mr. Bridges, the then District Officer, reported that when, at Agbani in 1924, an Akandri (one of the achondroplastic dwarfs collected by Aguku travellers) came through on his travels, the Government Ibo interpreter, though a native of Awka town only six miles from Aguku, was unable to understand the Akandri, who also had an interpreter. It was stated that these Akandri always use an *akolo* language when speaking.

*University of the Witwatersrand,  
Johannesburg*

M. D. W. JEFFREYS

#### Iron Gongs from the Congo. Cf. MAN, 1955, 30, 196

**20** SIR,—As a supplement to my article 'Iron Gongs from the Congo and Southern Rhodesia,' I would like to add the following:

Double iron gongs were introduced into Northern Rhodesia by invading tribes from the Congo during the seventeenth and eighteenth centuries. Some 20 miles east of Lusaka H. Neethling discovered a remarkable burial of the Soli chief, Kondola Bimbe, who was renowned as a blacksmith during the years of his chieftainship, between A.D. 1720 and 1750. He was buried together with his wives, slaves, weapons and other objects of chieftainship, which included ceremonial tongs, an iron stool, a bow stand and a number of iron gongs of Congo type. The material from this grave is preserved in the Rhodes-Livingstone Museum, together with double gongs of the chief of the Nkoya and of Chief Mulungwe of the We. All these gongs have been derived from tribes who migrated into

Northern Rhodesia from Angola and the Congo during the last two centuries.

I wish to thank Dr. J. Desmond Clark, of the Rhodes-Livingstone Museum, Livingstone, for allowing me access to this material and for supplying information.

*Maseru, Basutoland*

JAMES WALTON

#### Some Cart and Wagon Decorations. Cf. MAN, 1955, 126, 160

**21** SIR,—Arthur Young in his *General View of the Agriculture of Essex*, 1813, mentions the fact that two-wheeled carts were imported by sea to Essex from Edinburgh in the early years of the nineteenth century. The one illustrated in Young's book is similar to the type still in existence in East Anglia. Young said, 'They are cheaper (carriage included) than can be built in this neighbourhood, best put together and the nicest workmanship I ever saw, and infinitely lighter to the horses.' It is possible, therefore, that the spectacle design on the frontboards of vehicles was first introduced to the Eastern Counties through this importation of Scotch carts by sea to Essex. Gradually local wheelwrights took over this design and included it on the frontboards of their carts and wagons. The adoption of the design must, however, have been a gradual process, for as far as wagons are concerned, the design only occurs on the later plank-sided vehicles built in the latter half of the nineteenth century.

In addition, the spectacle design was limited to vehicles built in Eastern England. It occurs in the carts and wagons of Lincolnshire, East Anglia and the East Midlands, and as far as it is known it never occurred in the vehicles of Southern or Western England.

The widespread settlement of Scottish farmers in East Anglia in the mid nineteenth century may also have had a great effect on the construction and design of carts and wagons. Farmers from Scotland undoubtedly brought their own tools, implements and vehicles with them and undoubtedly also local craftsmen emulated the best in these designs. These two factors, therefore, the importation by sea of Scotch carts to Essex in the early years of the century, and the settlement of Scottish farmers in East Anglia, must have had a great effect on the technique of vehicle-construction in the regions concerned.

R. L. Edgeworth, also writing in 1813 on *The Construction of Roads and Carriages*, describes the various types of carts in use at the time. He describes the Scotch or Leith cart as used in Ireland and says that it was a vehicle with wheels four feet in diameter. It differed from two-wheeled carts in England in that it had hardly any sides, while the front and back boards were completely absent. The Scotch cart was introduced to Ireland some five or six years previously. If Edgeworth is correct, then the early nineteenth century did not see the introduction of the spectacle design into Ireland; indeed the carts of the period had no frontboards at all on which to paint such a design.

*University of Reading,  
Museum of English Rural Life*

J. GERAINT JENKINS

#### 'Slash-and-Burn' Cultivation. Cf. MAN, 1955, 144

**22** SIR,—I have read Professor Ekwall's interesting article under the above heading. While agreeing with your note that 'slash-and-burn cultivation' and *jhum* are not satisfactory as 'standard' nomenclature for the cultivating technique in question—the former being too clumsy and the latter too exotic—may I say that the suggested neologism seems to me superfluous, as a perfectly satisfactory term already exists in the German *brandwirtschaft*? Apart from the obvious advantage in adopting a term already in standard usage among German-speaking anthropologists and archaeologists, it certainly seems preferable to use a word in a modern European language of which the overwhelming majority of workers in these disciplines have at any rate a reading knowledge rather than to introduce one derived from an obscure dialectal source with which probably few, other than philologists, are familiar. I would add that *brandwirtschaft* may fairly be said to be on the way to 'acclimatization' into English archaeological writing since its consistent use in J. G. D. Clark's monumental *Prehistoric Europe*.

*Bristol*

R. D. GREENAWAY





(a) The large rock gong at Dutsen Murufu at Birnin Kudu, on which seven different notes were identified. The lip of the rock spall has been worn to a horizontal position by continuous hammering.



(b) Cave adjacent to that shown in (a) at Dutsen Murufu showing two large multiple gongs and smaller ones in the shadows behind.



(c) View of Dutsen Mesa with painted rock shelter protected by steel screens and masonry. Three rock gongs are marked X. The gallery shown at the bottom left hand corner gives access to other multiple gongs.



(d) Rock gongs at Mbar, exposed to the weather. The lichen does not cover the hammered depressions.

#### MULTIPLE ROCK GONGS IN NIGERIA

Photographs: B. E. B. Fagg

# THE DISCOVERY OF MULTIPLE ROCK GONGS IN NIGERIA\*

by

BERNARD FAGG

*Jos Museum, Nigeria*

**23** The first recorded discovery of ringing rocks used for the production of musical notes was made at Birnin Kudu in Kano Emirate (9° 30' E., 11° 30' N.) in June, 1955, during field investigations of the recently discovered rock paintings, the first to be found in Nigeria.<sup>1</sup> The term 'rock gong' is here used to denote the small percentage of the numerous natural rock boulders, slabs, spalls and exfoliations abounding in the rocky hills of Nigeria which vibrate with a ringing tone when struck and which also show indisputable evidence of having been used as percussion instruments.

The rock gongs at Birnin Kudu were found in an archaeological context, for their distribution in relation to the seven painted caves so far discovered leaves little doubt that they are associated in some way with these sites. The rock gongs themselves are usually clustered within about 100 feet of the paintings, whereas ringing rocks which have not been used are to be found scattered all over the outcrops of granite, which cover an area of approximately two square miles in the gently undulating plains of this part of Kano Province.

At Dutsen Mesa where the first and most important paintings were found there are no less than ten groups of rock gongs within about 100 feet of the rock shelter. However, the most spectacular group of gongs was found at Dutsen Murufu in two small caves about 20 feet behind the rock shelter. Here we discovered a very faded though remarkable painting of a dwarf shorthorn bull, a species now extinct in this part of the country. We later discovered that the brides of Birnin Kudu (now a staunchly Mohammedan community) go to this cave at dawn on their wedding day and return in the late afternoon to the town for the marriage ceremony. As they are apparently oblivious of the paintings it seems likely that this custom may be a vestigial survival of rites, now forgotten, which were most probably connected with the paintings and the rock gongs a few feet away. The children of the town at present play at hammering on these gongs and in most places it is possible to distinguish their fresh chatter-marks, usually close to the ground, from the old patinated depressions, which, incidentally, are frequently out of the reach of children. The information given to me was that there is no local knowledge of any ceremonies ever having been carried out at this place or in connexion with any of the rock gongs, but this evidence will require more searching investigation.

The gongs at Dutsen Murufu were very probably used as ensemble instruments<sup>2</sup> by a number of players, for otherwise it is difficult to explain away rather more than 50 hammered depressions on a dozen different rocks of which half weigh no more than a hundredweight or two,

\* With Plate B and a text figure.

and half are huge masses of rock weighing several tons apiece. The size of the rock apparently bears no direct relation to its tone, but the rocks must be free to vibrate in order to ring and not touch the solid earth at any point. The rocks themselves are almost always absolutely solid; when they have incipient exfoliations they give a hollow, less metallic tone.

With a borrowed set of tuning forks, and relying on the ears of the two best drummers in the town each depression

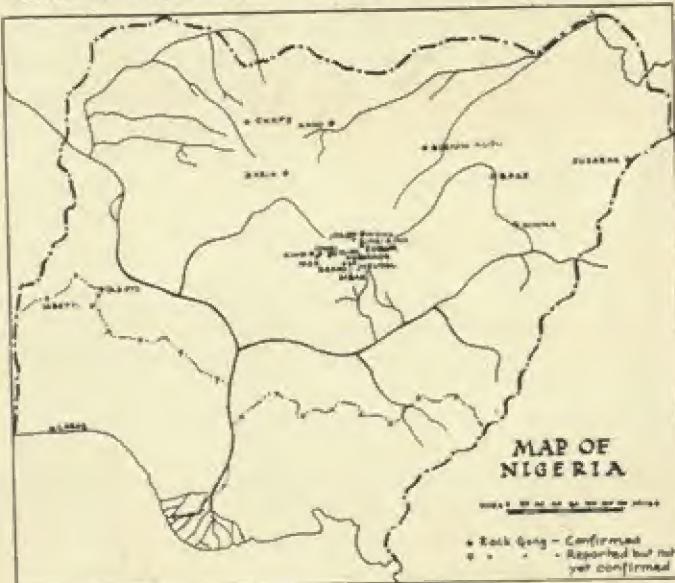


FIG. 1. DISTRIBUTION OF ROCK GONGS

was tuned and marked with the approximate note or semi-tone. Eleven of the notes were identified in this way, and the other two were found by checking back over the biggest of the multiple gongs, on parts of it which had been only slightly used. This result of course proves little except that a detailed musical analysis would be worth attempting.

At Birnin Kudu all tests were carried out with the use of wooden hoe handles to avoid bruising the surface of the rocks, and possibly spoiling the evidence of their antiquity. Tape recordings were made of a group of five local drummers beating out all their local drum rhythms. After practice they were able to simulate the drumming of all the five drums known to them, including even the hour-glass-shaped *kalangu* or talking drum.

In September I visited Rui, near Bokkos (50 miles due south of Jos), to re-examine a rock I had casually seen there in 1940 with certain depressions which I had found it difficult to interpret at the time. I found the rock deeply buried on a shrine, but the visit led to the discovery of rock gongs at Bokkos, Jukudel, Mbar and Daffo, which were found to be in current use. They are invariably struck with fist-sized lumps of stone. They are called *kwangalang* in the

Ron dialect at Bokkos, and *kongworiang* at Daffo, onomatopoeic names which make it perhaps not surprising that I next found rock gongs at the village of Gwong and at the hamlet of Gingiring a mile or two to the east of Jos itself. These were well patinated and not known to the Jarawa inhabitants, who are, however, familiar with rock gongs at the village of Fobur, some ten miles to the south-east, where the Jarawa initiation rites take place.

Rock gongs were found and recorded at Kwoi, Nok and Chori in southern Zaria Province. They have indeed been found in hilly country wherever a determined effort has been made to locate them.

I have in addition received unconfirmed but reliable reports of these instruments being used also in the following places: near Chafe in Sokoto Province, at Kufena near Zaria City, at Shira Hill in Katagum Division,<sup>3</sup> at Bage near Nafada, at Kusarha Hill near Gwoza in the Northern Cameroons, at Hinna in the lower Gongola valley, and most recently at Old Oyo and Igbetti in the Western Region.<sup>4,5</sup> I have also had a report from Mr. M. H. V. Fleming of the Education Department that, when in the Sudan in 1929, he noticed a rock with a number of cup-like depressions in a village to the north of the Sixth Cataract on the west bank of the Nile, which on enquiry was described to him as a bell.

The contemporary uses of these rock gongs varies greatly though they are most frequently used in secret religious ceremonies, often in connexion with circumcision at initiation rites (Mbar, Bokkos, Daffo, Fobur). They are used at Nok in the ceremonies just prior to the harvest of the first *acha*<sup>6</sup> when certain grass seeds are carried up to the cave by the unmarried girls and ground on the solid rock. Here and in several other places the gongs are closely associated with corn-grinding grooves worn down into the solid granite.

At Kusarha Hill in the Northern Cameroons they are said to be used for communicating with spirits whose reply is received in the form of echoes from the depths of the cave.

At Nok and elsewhere in Jabaland they are said to have been used as warning signals of the approach of Fulani cavalry during the Holy Wars of the nineteenth century, and indeed the sound will carry up to two or three miles in favourable conditions.

They are in addition used in many places also for merry-making, for they provide an excellent accompaniment for singing and dancing, resembling in sound and rhythmic use the conventional double hand-gongs of iron,<sup>7</sup> which are so widespread in Equatorial Africa. They are frequently closely associated with rock slides, sometimes as long as 150 feet, on which the boys amuse themselves by sliding down on small rock sledges from the tops of bare granite hills.

It is somewhat surprising that these rock gongs have escaped serious notice for so long. It is still premature to speculate on their origin, but it is possible that they came into current use with the introduction of blacksmithing, for most African smiths use anvils of solid rock. It is equally possible that they may eventually prove to stretch back into the remotest antiquity and to have been among men's earliest musical instruments, for the men who depended for life itself on their ability to fashion implements by flaking must have been conscious of the musical quality of stone.

#### Notes

<sup>1</sup> It is hoped that a description of the paintings and preliminary excavations will appear in a subsequent issue of MAN.

<sup>2</sup> This was discussed with Mr. Hugh Tracey at the African Music Society's Headquarters near Johannesburg, who confirmed that this is the most likely explanation.

<sup>3</sup> These rocks were seen many years ago by Mr. W. F. Jeffries, Director of the Northern Region Literature Agency, Zaria.

<sup>4</sup> This was reported by Dr. Morton Williams of W.A.I.S.E.R., Ibadan.

<sup>5</sup> A similar ringing rock was shown to Father Kevin Carroll on a visit to the Nok Hills in 1950.

<sup>6</sup> A diminutive cereal crop confined to the hill regions.

<sup>7</sup> The name for 'rock gong' in the Ham (Jaba) language is *Kuge*, which is merely the Hausa word for the double iron hand gong.

## INDIANS IN AFRICA

by

DR. L. HOMBURGER

*Directeur d'Études à l'École des Hautes Études, Sorbonne, Paris*

**24** In his *Prehistory of Uganda Protectorate* (C.U.P., 1939) T. P. O'Brien says: 'The Stone Age African realized his own and essentially African civilization with African material.' I have no personal opinion about the palaeolithic African, but I was struck by O'Brien's observation because many linguists seem to have had the same feeling about the development of Negro African languages.

I do not wish to criticize those who have done much to encourage the study of the said forms of speech; my

object in writing this article is to point out a certain number of facts which show that the Africans do not use languages made entirely in Africa, though they have evolved in that continent for some centuries. It is generally recognized that the modern Pygmies and Bushmen of the Equatorial forests and the Kalahari desert are survivals of races which were spread over the whole of Africa before the arrival of Mediterranean peoples in the North and of darker-skinned people in the East.

Early Egyptian documents mention Pygmies famed as

dancers, who were brought from Punt by sea, or from the Upper Nile. According to Lebzelter the Bushmen represent a mixture of several ancient races. The rock paintings of South Africa are generally recognized as being their works, and those found in the Egyptian Sudan and in Algeria are so much like the first mentioned that most anthropologists consider that the Boskopoids or ancestors of the Bushmen once occupied the north of the continent. The brown or black populations seem to have invaded Africa from the east and traditions point to the regions east of the Upper Nile as being those from which they spread south and west. As the populations of South India are black and as the currents of the Indian Ocean go north along the Malabar Coast, turn west along the south coast of Arabia and then go south, there is every reason to agree with the eminent French anthropologist, Professor Vallois, who thinks that the African Blacks came from South India.

The very few physical characteristics which distinguish modern South Indians from modern Africans are due to mixture with different races in the course of three or four thousand years. The exact date of the arrival in Africa of black peoples is a much discussed point which it is not necessary to enter into here. It is more important to recall what is known about the sea routes between ports on the coasts of Africa and ports in Asia. According to the eminent Egyptologist Wallis Budge, author of *A History of Egypt*, the Egyptians of the First Dynasty came from South Arabia by the Red Sea to a port now called Kosseir at the mouth of the Wadi Hammamat. They went up the valley forming the bed of this river as far as Coptos and reached the Nile. Budge thinks that some of their compatriots went west and founded the kingdom of Punt near Cape Guardafui about the same time (c. 3,000 B.C.), for Naville pointed out that in early Egyptian documents the name of this country is not accompanied by the sign normally employed with names of foreign countries.

As Budge's work is dated 1902, he knew nothing then about the great Indus civilization of Mohenjo-Daro and Harappa described in Sir John Marshall's monumental work *Mohenjo-Daro* (1927). As burnt bricks were used there and as the Egyptians of the First Dynasty appear to have introduced them to their predecessors in the Nile valley, it seems likely that they owed their distinctly superior civilization to contact with that of the Indus. Budge specifies that Egyptian and Semitic languages have many elements alike but are distinct branches of a common stock; it is probable that in future times it will be recognized that the origin of both was in India, but for the present this hypothesis need not be discussed here.

The peculiar importance of South Arabian civilization in Africa was pointed out in 1939 by an Italian archaeological mission, the members of which declared that traces of it were associated with the earliest signs of man. Some writers imply that the passage to Africa from Asia must have been by land or across the straits of Bab el Mandeb, but recent researches show that the sea has not been an obstacle for some thousands of years. It is not necessary to

recall the different expeditions sent to Punt by the Pharaohs, but the following points are important:

1. Egyptian gods have pointed plaited beards; the kings of the First Dynasty have such beards, but their successors are shaved. When Queen Hatshapset sent an expedition to Punt, the inhabitants of that country are represented with such beards.
2. An Egyptian expedition that had gone up a river beyond the straits is depicted in a port the inhabitants of which are of two distinct types: some are like the modern dark Abyssinians (Budge, Vol. IV, p. 6) while others are fine men with pointed beards.
3. The religious importance of the *ka* in Egyptian is well known though no satisfactory translation has been found; in South Indian Dravidian languages we find Kanara: *ka*, soul, spirit = Tamil *ka*, 'Brahma, Agni.'

This last point does not seem to have been noticed; *ka* is one of a certain number of words common to Egyptian and Dravidian which I have noted. With the exception of Christianity and Islam, Indian religions do not seem to have come from the west.

Punt is the only country beyond the straits mentioned by name in connexion with trade in old hieroglyphic texts.

Indian inscriptions and documents other than the undeciphered seals of the Indus civilization do not go back further than the second millennium B.C. There is every reason to suppose that wood and perhaps cotton or silk used by scribes have been destroyed by ants. The absence of iron tools may have long been an obstacle to inscriptions on stone such as those that appear a few centuries B.C.

Fortunately Alexander ordered Nearchus, one of his generals, to explore the coasts of the Persian Gulf and a minute description of the journey has been preserved in Arrian's *Indica*; from it we learn that the said coasts were inhabited by peoples of different races and languages and that there were ports visited by traders.

*The Periplus of the Erythraean Sea* written by an anonymous author (probably Egyptian) shows that Egyptian merchants were in the habit of circulating on the Green Sea (i.e. Indian Ocean). There were three routes to India from the Egyptian ports on the Red Sea: (1) by coasting along Arabia and the shores of the Persian Gulf to Barugaza on the Narmada; (2) from Kanê on the Arabian coast to Muziris and Nelkunda on the coast of Malabar; (3) from the neighbourhood of Cape Guardafui to the same ports. The approximate date of this work is A.D. 80. We learn from the *Periplus* that Ptolemy Philadelphus had built the port of Myos-Hormos on the Red Sea about 274 B.C. to replace Arsinoe, which had been the starting point for traders going to India or coming from there. The author also states that Berberts (cf. Berbera) transported African products to Arabia by rafts. In another passage, he says that the coast of the Barbarin is beyond the Cape of Aromates (Guardafui). The mention of this name suggests the following remarks: (1) the modern Nubians call themselves Barabra; (2) the At-barra is an affluent of the Nile beyond Nubia; (3) besides Berbera on the coast there are Barea and at least two Bari tribes in the Upper Nile regions (the name is said to mean 'foreigner'); there are Beriberi in Bornu and Bari is a favourite name among the Fulani.

The Sanscrit name for India, *Bharate*, might have been

derived from Dravidian *bara* or *vara*, corn (wheat or millet according to the dialect); Sanscrit *abhir*, pastors, may explain *Beiras*, *Beir*, etc. Is it not possible that in Africa the word *bar* meant 'Indian' about the beginning of our era? However, identification of proper names is always a source of possible errors and I am not quite certain that the one just suggested is correct.

Nikon, a port on the coast of Africa mentioned in the *Periplus* (called by Ptolemy *Tonike*), has been connected with a Bantu tribe called *Nyika*; the latter means 'coast' and *Nikon* renders Bantu locative *Nyakani*; the coast was inhabited by Bantu-speaking people, but not necessarily by the modern *Nyika* or *Ba-Nyika*. *To-nike* is an Egyptian form of the same name, *to* meaning 'country.'

The classical geographers Ptolemy, Strabo and Pliny mention numerous names of towns in India (L. Renou and J. Filiozat, *L'Inde Classique*, p. 245), and the Tamils of South India developed in our era the flourishing commercial relations which they had long kept up with South Arabia, the Red Sea and the eastern coast of Africa. When Vasco da Gama reached Mombasa, there were Indians established there and it was an Indian pilot who took him to Goa.

The coast of Malabar has always had a surplus population and invasions from the north, religious persecutions and famines have led Indians for centuries to seek safety and an easier life in Africa. The Brahmins disapproved of travel by sea, but Christians belonging to churches founded by St. Thomas and his disciples had no such scruples and it is known that Abyssinia was converted by two young Christians travelling from India to Alexandria.

The Dravidians were not disciples of Brahma in the first centuries of our era; hence the fact that H. Baumann in *Volkskunde Afrika's* (1940; French translation, 1945) says that the New Sudanic civilization was wholly introduced from South India (autocratic kings with pages and courts in which the Queen-Mothers played a part; armoured horses, parasols borne over or before the sovereigns, etc.).

My own linguistic researches have led me (quite independently) to conclusions which tend to confirm those based on ethnological facts.

Groups speaking Dravidian languages have landed on or near the coasts of the modern Somalis in the course of the last 2,500 years. One set, speaking Kanarese (=Kannada) organized a state somewhere between the coast and Lake Victoria and the language spoken there comprised morphological elements used by the dominant class and lexicographical elements of various sources. After a length of time sufficient to amalgamate the diverse parts this state was broken up by the arrival of warrior tribes, and spread along the coast as far as the Limpopo, westward to the north of the Equatorial Forest and southwards towards Lake Nyasa. Being only in contact with Hottentots and Pygmies these people preserved their name *ntu* and their language generally, only with the slight differences due to the law of change which dominates all human speech.

As *ntu* was the correct name for India according to a

Chinese traveller—Sanskrit *sind*, Persian *hind*, Greek *indoi*—and as formerly the Blacks of South Africa used only *mu-ntu* (pl. *ba-ntu* 'men') for themselves, we consider that Dr. Drexel's surmise as to the names is correct. (The fact that *ki-ntu* may mean an 'object' is probably an accidental homonymy or an evolution such as has given French '*rien*', 'nothing,' from Latin *rem*, 'a thing'.)

In the so-called Cushite zone of North-East Africa, in the Nile valley and in all the Sudan from the Nile to the Atlantic Ocean there are a few 'countries' in which the clans and tribes speak languages which are easy to recognize as being distinct one from the other; such are Nubian, Kanuri (Bornu), Hausa, Mande, Wolof. But the differences do not prevent the recognition of common elements; a careful study has led most linguists to the conclusions first formulated by me in 1913: all Negro-African languages have a common basis.

The comparison of these languages with different Dravidian languages has led me to modify slightly my former definition: the modern Negro African forms of speech represent morphological forms of one or several Dravidian dialects introduced by an Indian dominating group into the language of their subjects. The proportion of words is impossible to calculate, for (1) there may have been two successive sets of Dravidian masters; (2) if all the Blacks came from India, words now used by Dravidians may have been brought over some centuries or even millennia earlier and have belonged to the vocabulary of Proto-dravidians.

The facts which have led me to adopt conclusions in accordance with those of anthropologists and ethnologists have been set forth in my articles 'Éléments dravidiens en peul' and 'Les telougous et les dialectes mandés' (*J. Soc. Africaines*, Vols. XIX and XXI) and at the International Congress of Linguists, London, 1952.

There is still much to do ere the Dravido-African field has been explored as thoroughly as the Indo-European has been today, but there is no lack of materials.

Ethnologists such as Hornell, Huntingford, etc., have realized that there have been connexions but not that the modern languages prove them to have been constant. I hope that my work will be welcomed by them.

Professor Hutton, who declared that the tin mines of Nigeria had been worked by Indians, was certainly right, but it is difficult to fix a date. The penetration which led to organization of states and languages in West Africa was probably anterior to the Indian traders whose presence at Timbuktu was known to Arab writers; whether they or the colonists introduced cowries is a question which need not be discussed here, any more than those relating to humped oxen, cotton, etc.

When one reads an article such as Lieut. Francis Wilford published in *Asiatic Researches*, Vol. III, Calcutta, 1792, one realizes that the Indians knew more about the Nile and East Africa than Europeans at that date ('On Egypt and other Countries Adjacent to the Cali River or Nile of Ethiopia from the Ancient Book of the Hindus'); it is not possible to cite it at length here, but it is to be hoped that some Indian scholars will give us another paper with

complementary evidence and identification of more names. The author considered the mention of snow-capped mountains a proof that the documents were not reliable.

I hope that this brief sketch of the evidence relating to connexions between India and Africa will lead many readers to study the facts which I have only touched on; the linguistic evidence throws light on many problems; the religions of old India will probably help the students of African pagan cults, and many names of people and places

in West Africa are found in East Africa and in India, cf. *Canara Kongu*, *Mandingo Kong*, *Bantu Kongo*.

If the following statement should prove to be correct, it would be a strong confirmation of all that we have said: 'Jordanus, a monk writing in 1330, said that "East Africa south of Abyssinia was called *India Tertia*"' (cited by W. R. Halliday, *Folklore Studies*, London, 1924). Our linguistic studies do not allow us to pursue the subject further.

## KINSHIP AND FARMING IN WEST CUMBERLAND

by

W. M. WILLIAMS

*University College of North Staffordshire*

**25** Among the farmers of West Cumberland, 'everybody is related to everybody else.' This paper attempts to show how the structure of the farm families and the larger groups of kindred of which they form part is related to the occupation and use of the land.

Most of the farms of West Cumberland are 'family farms.' The farm family is the unit of economic production. In the Gosforth district nearly three-quarters of the male labour on farms is provided by farmers, their sons, and other relatives; 95 per cent. of the female labour is provided by farmers' wives, their daughters and other relatives. Moreover, the ideal of the family farm—that is, one that can be run without the help of hired labour—is so strongly developed that it appears to override considerations of simple economics and efficiency. It is not uncommon to find holdings which are barely holding their own in quality because the farmer and his family cannot provide sufficient labour to keep the land in good condition. On most farms the absence of hired labour implies that everyone works very hard. School-children are expected to do two or three hours' work in the evening, and the village schoolmasters have long been accustomed to children being absent because their fathers needed them to lift potatoes or help in the sheep-dipping or the harvest. This state of affairs has been exacerbated in recent years by a severe shortage of hired labour. Those farmers who are willing and anxious to employ men find it extremely difficult to engage them. This has enhanced the importance of the family as the unit of production and is important in understanding the detailed working of the family organization.

It will be clear that in most cases every member of the family has a part to play in the running of the farm. Since the average family is small—three sons are exceptional nowadays—since they are in school until they are fifteen and since they do not live with their parents when they marry, it is not surprising that the average marriage age of farmers' sons is later than that for the country as a whole. In Gosforth, nearly half of them married between twenty-five and thirty, a quarter between thirty and thirty-five and an eighth after they were thirty-five. The countryfolk are

well aware of the implications of this. Farmers remarked often that they had not married sooner 'because I wanted to help me folks' or that 'I were too busy when I were a lad.' The example of a man of sixty who married in 1951, a year after the death of both his parents, was often cited as an instance of 'looking after his folk.' While this is an extreme case it points to the loyalty which is expected of children.

A family composed of parents and children only, which is also the economic unit, has a characteristic life cycle. Its fortunes vary with its development. At first, when there are no children, or when they are very young, life is hard and the help of paid labour or kindred is necessary to run the farm. As the children grow older, conditions become increasingly better until they reach an optimum when all the children are grown up and working for their parents. Then as the children marry the burden on the parents becomes heavy once more, and old age or illness may once again necessitate help from outside the family.

The vital factor in this situation is clearly that children do not live with their parents when they marry. Where they live depends on the pattern of inheritance. Non-inheriting sons leave home upon marriage, usually as farmers in their own right. The inheriting son stays at home as a bachelor and is not free to marry while his parents are alive.

In the case of non-inheriting sons an attempt is made by the parents to buy a farm or obtain a tenancy for him. There is no marriage bargain, but very often there is an informal arrangement which serves much the same function. In one instance the two sets of parents contributed to buy a freehold farm. In others the young man's father bought the land and the girl's father supplied some of the stock and machinery. There is considerable variation in the extent to which each family contributes, and this is not always a direct reflection of means. Prestige plays an important part. In general, however, the bridegroom's parents contribute most—usually in land, stock, equipment, etc.—while the bride's family confines itself to household goods.

This traditional arrangement has been much complicated in recent years by the constantly increasing difficulty of

finding suitable farms, especially those on lease.<sup>2</sup> The effect of this has been to postpone further the age of marriage of non-inheriting sons and to emphasize the bonds of loyalty that bind them to their parents. Occasionally, young men have been able to rent cottages near their parents' farm. This has enabled them to marry and set up their own home, but they continue to work for their father until a farm becomes available. This is not a very common practice, partly because suitable cottages are scarce and partly because it is viewed with disapproval by many people. In the words of one farmer, 'There's time enough to be getting married when tha's got a proper spot (farm). Some folk might think they was in a hurry to leave home.' Most farmers' sons are well aware that their prospects of being settled on a holding when they wish to marry are becoming steadily poorer, and a few have seized the opportunity to start farming on their own even when this is at the expense of their family. The younger of two sons, aged twenty-two, who deprived his family of much needed labour in this way was said to be 'very ungrateful,' 'thoughtless,' and 'a poor mak (kind) of son.' Generally, however, non-inheriting sons live and work at home for 15-25 years or more. They think of this period of their lives as a payment of the debt they owe their parents for bringing them up and for helping them to find a farm and to set up on their own.

The inheriting son is in a quite different position. His reward is the family holding and everything on it. He is regarded as a very favoured young man, since the farmers of West Cumberland place a very high value on the ancestral holding. Many families have farmed the same holding for three or four centuries, or even longer.<sup>3</sup> It is not, therefore, regarded as a burden to remain a bachelor until 'early middle age.' One farmer's son in this position (he was thirty-eight) remarked: 'When tha's born to a spot tha's married to it.' The normal procedure is for the farmer and his wife to farm until they feel that the time has come to retire. This is rarely before the father is sixty-five, and very often much later. When they retire they move into a cottage or house they have bought or rented in the district. The heir then becomes the owner or tenant and is free to marry. His new status is locally recognized by a change in nomenclature; he now becomes, say, 'Robert Sherwen, Beck End' instead of 'Andrew Sherwen's lad.' In general, marriage follows inheritance very quickly—on occasion during the same week. It seems probable that the farmer tells his heir of his decision to retire some months in advance.

(It is tempting, if dangerous, to relate this to the fact that the first child of a farmer's marriage is born quite frequently within six months of the ceremony. In a sample of nearly 100 farmers' marriages, the first child was born within four months of the ceremony in just under a quarter of the cases, and in the first eight months in over half the cases.)

The pattern described above is, however, the ideal and is often interrupted in practice by the death of one of the parents. If the father dies, the heir stays at home unmarried during his mother's lifetime, since the prohibition against

marriage is essentially directed against the mother and daughter-in-law living together. Local opinion was unanimous that 'it would never work.' Farms occupied by a widow and her bachelor son or by a bachelor who was 'past it' when his widowed mother died are quite common. (Where men remain bachelors due to a situation of this kind, rather than from choice, it is locally held that they become 'woman keen'. This is the term used to describe a psychological state in which, it is alleged, a man is greatly attracted to women in general and at the same time unable to approach them in any way. The internal conflict which results is widely believed to make such men 'as daft as a brush.' Some of them did indeed appear to be rather odd.)

If the mother dies first and the father remarries, the heir is still not free to marry. As long as his father has a wife, he must remain a bachelor. The extreme example of this was that of the farmer who remarried twice, the second time when he was nearly sixty; his son, who is older than his present step-mother, 'never bothers to think' about marriage. If the mother dies first and the father does not remarry, the heir is free to marry when his father retires. It is customary for a widowed father to find a small cottage for his retirement, possibly with an unmarried daughter to look after him. The consensus of opinion was that a widower should hand over to his son as quickly as possible, even if he stayed on in his old home and helped in the running of the farm.

In all these variations it is clear that the central principle is that of maintaining the family as a relatively self-sufficient economic unit and the retention of the holding within the family. The way in which these ends are achieved means that the elementary family is the most common type on West Cumberland farms and makes true three-generation families correspondingly rare. On farmsteads where there are three generations, the family is most often composed of either a widower with a married son or daughter and their children, or parents with unmarried daughters with illegitimate children. Daughters usually leave home when they marry: the exceptions to this occur in families where there are no sons to inherit (and where, therefore, a son-in-law's help is economically important) or where the farm is run by a widow.

A number of farms are occupied by unmarried siblings. All the cases investigated were the result of the mother surviving her husband for a relatively long period; the children who stayed at home to run the farm were 'too old' to marry when she died.

Other features of the family organization underline the cardinal importance of 'keeping the spot in t'family.' When, for example, a farmer is survived by a widow, she normally inherits the holding. It remains her property until she dies, except in rare cases where it is transferred to the male heir during her lifetime. It seems that the heir is chosen while the father is alive—in some instances even before all the children are born. It is a favoured practice to incorporate special clauses in wills to ensure that a widow does not change the succession decided on by her husband.

There is no fixed pattern of inheritance. An analysis of

nineteen farms which have been occupied for at least three successive generations showed that:

- In 6 cases the youngest son inherited.
- In 18 cases the eldest son inherited.
- In 9 cases the only son inherited.
- In 8 cases there was joint inheritance by two sons.
- In 4 cases there was joint inheritance by other siblings.

Other variations encountered were inheritance by the second eldest son of three, by daughters in families where there was at least one son, and by other kindred in childless 'families.' The preference for primogeniture and the pattern of elementary families reinforce each other in delaying the marriage age of sons who inherit.

Remarriage appears to be fairly common in the farming communities of West Cumberland. It can be stated as a general rule that if a farmer or his wife should die, the surviving partner will remarry if he or she is aged forty-five or less. (There are, of course, exceptions; equally there are often remarriages after the age of forty-five.) This is consistent with the aim of maintaining the family as a functioning economic unit. As one farmer put it: 'When there's lads on a spot<sup>4</sup> there has to be a mother and father.' When it is the wife who remarries there are invariably provisions to ensure that the holding remains in her first husband's family.

In a family system of this kind it is not surprising that paternal authority is well developed. The father is head of the family in all respects and his children are economically and psychologically dependent to a very marked degree. One farmer described how he had worked at home until he was forty before he received any 'pocket money.' 'If,' he said, 'I wanted a penny bun, I had to ask me father—and likely enough he said no.' This is by no means atypical; most farmers seem to have experienced the same sort of treatment. It was said that the subordination of farmers' children was so extreme 'in the old days' that several had committed suicide rather than submit to further repression. This, however, did not apparently apply to the present. Nowadays, it was commonly said, everyone is much more lenient—'lads and lasses is proper spoiled today.' Yet it is still true that many farmers' sons and daughters receive no pocket money or wages, despite the official rulings of the Agricultural Wages Board. If they want money they have to ask for it and explain fully why they need it. They are much restricted in their movements and while they remain at home they are given little opportunity to assume the responsibilities of adults. The following example illustrates some of the effects of this. Three sons, the youngest thirty-one, the oldest thirty-six, inherited a farm jointly when their father died very suddenly in 1951. The confusion that followed was a topic of conversation for months afterwards whenever farmers met in any number. Since the farmer had regarded his sons as 'just lads' they had been told virtually nothing about the running of the farm and had great difficulty in its management for a long period after his death. One of the sons, who had rarely left the farmstead while his father was alive, became a familiar figure in a nearby village as he went to the bank in search

of further information concerning his father's financial affairs. Another took on the task of attending the sales at Whitehaven and visiting the firms who supplied the farm. 'Poor lad,' said one farmer, 'he didn't know who he had to see, and when he knew that, he didn't know where the office was, and when he got there he had nowt to say.'

This dependence is not resented by the younger generation. It is part of the natural order of things. Like loyalty to one's parents it is a *sine qua non* of a system in which the rewards of an assured economic and social status are only rarely the lot of a young man. Both are of great importance in ensuring the stability and continuity of the system, and at the same time in reducing the possibility of change to a minimum. Men accustomed to half a lifetime of obeying their father are unlikely to initiate drastic innovations.

The term 'family' is used in West Cumberland not only to refer to a household but also to a larger grouping—that of grandparents, parents, brothers and sisters, children and grandchildren.<sup>5</sup> In the latter sense it is used to distinguish this group from the remaining kindred, who may be 'close relatives' or 'distant relatives.' A man's total relatives form his kinfolk, and he may have from twenty to over a hundred living around him.

The detailed functioning of the individual farm family is much influenced by the fact that it is part of the larger grouping of kin. As we saw earlier, kindred are often called upon to help when the farm family is unable to provide all the labour necessary to run the holding. In the same way the kin group functions as a unit on those occasions when the work demands relatively large numbers of men working together, such as the harvest during periods of bad weather, on threshing days, sheep dipping and shearing, etc. Farmers who are kindred frequently borrow and lend machinery amongst themselves, and so the amount of expensive equipment a family needs to acquire is in part determined by the equipment owned by kinfolk. This also helps to reduce the financial burden on the farmer whose son is beginning to farm on his own account. His relatives can always be relied upon to lend a binder or cultivator when the occasion demands. In times of financial stress kindred offer the first and best hope: unlike banks they do not have interest rates.

Much of the strength of the various kin groups which make up the farming community arises from the fact of 'everybody being related to everybody else.' West Cumberland farmers are almost an endogamous group. Moreover, marriage frequently occurs within a given kin group or, more commonly, between the same two kin groups over a period. In Gosforth, for example, at least 12 of the farmers' marriages in recent years have been between 'close relatives'; seven of them were marriages between first cousins. Equally popular is the practice of marriage between sets of siblings. In Gosforth there was one case of three brothers marrying three sisters, one of two brothers and a sister marrying two sisters and a brother, and five cases of two siblings marrying a pair similarly related. Unions of this kind obviously serve to form firm alliances between kin groups and families, and to add to the complexity of the kinship system.

Kin groups have a name, a collective reputation and they are capable of united action. Although the individual families of a kin group are scattered over a wide area, each group has a recognized centre—the farmstead which is traditionally the ancestral home. Thus there are the Sherwens, characterized always as big handsome men and with high status among farmers. When referred to collectively they are described as the 'Sherwens Tarnhow,' and so 'John Sherwen Kemplerigg' and 'Peter Sherwen Bank House' are each 'one of the Sherwens Tarnhow.' This kind of nomenclature epitomizes much that is fundamental to kinship and land holding in West Cumberland. Each family, considered individually, is associated with a particular farm; as in Eire, they have their 'name on the land.'<sup>6</sup> But each family is also part of a larger grouping and its individuality is subordinated to the claims of this larger entity. Moreover, it is the 'original' holding that is associated with the kin group, and it is a striking commentary on the importance of allodial land that the farmers of West Cumberland are astonishingly accurate in this respect. The history of several kin groups was investigated and only in one case was there documentary evidence readily available (in farm deeds). In all the other kin groups the farmers knew of the ancestral holding 'because me

father tellt me' or because 'everybody knows we came from there.' Their accuracy could only be tested by considerable research into Parish Registers, documents at the Public Record Office and more recondite sources, and in every case it was the earliest recorded holding which was claimed to be the home of the group. However unreliable 'folk memory' may be in other contexts, here it worked very well indeed.

#### Notes

<sup>1</sup> This paper is based on field work in the parish of Gosforth from July, 1950, to February, 1952, and in several neighbouring parishes in the summer of 1953. The full results will be published later this year in *The Sociology of an English Village: Gosforth* (London: Routledge & Kegan Paul) (Chicago: The Free Press).

<sup>2</sup> For a further discussion of this point, see 'Some Social Aspects of Recent Changes in Agriculture in West Cumberland,' *Sociol. Rev. New Ser.*, Vol. I, No. 2, December, 1953, pp. 93–100.

<sup>3</sup> These are invariably freehold farms. It should be noted that tenancies are 'inherited' and there are numerous examples of tenant farms which have been occupied by the same family for a century or more.

<sup>4</sup> It must be remembered that 'lads' are often men of forty or older.

<sup>5</sup> See Fig. 1 in 'Kinship Structure in an English Village,' *MAN*, 1952, 208.

<sup>6</sup> Cf. C. M. Arensberg, *The Irish Countryman*, 1950, Ch. III.

## SHORTER NOTES

**Mycenaean and Indo-European.** By Stuart E. Mann, School of Slavonic and East European Studies, University of London

**26** In the nineteen-twenties Miss F. M. Stawell examined the Cretan hieroglyphic and Linear A inscriptions published by Sir Arthur Evans in 1909 (*Scripta Minoa*, Vol. I), and attempted to interpret the symbols as derived acrophonically from the representation of common objects. In this way she hoped to establish phonetic values for the signs, working on the assumption that the language was some form of Greek. In 1927 A. E. Cowley tentatively identified the word for 'total' or 'altogether' on several well-known inventory tablets written in the later Linear B script, since the word was followed by a numeral expressed in strokes. In an article in *Studies in Aegean Archaeology Presented to Sir Arthur Evans* he proposed identifying the words for 'boy(s)' and 'girl(s)' with Greek κούρος and κόρη respectively. Little attention was paid to these ingenious early attempts to break into the 'Minoan' script.

The late Bedřich Hrozný, discoverer of Hittite, went sadly astray in his attempts on 'Minoan' Linear B, and his work, like that of his predecessors, lacked the documentary material and the long preparation needed for a successful breach.

In the fourth volume of his *Palace of Minos* (1935), Sir Arthur Evans published several examples of the Linear B inscriptions. In them he recognized 'man-signs,' 'woman-signs,' numerals, and the word for 'total,' but did not attempt to give them phonetic values. On this scanty material an American scholar, the late Dr. Alice Kober, established the necessary preliminaries to decipherment by drawing up lists of signs now recognized as syllabic. These she began to arrange in the form of a 'grid' in such a way that the signs believed to be cognate as to their consonant were placed in lines, while those believed to be cognate as to vowel were placed in columns. The 'grid' was purely functional, and took no account of phonetic values. Her results were published in the *American Journal of Archaeology* between

the years 1943 and 1950. Like J. Knudtzon, who in 1902 discovered the athematic Indo-European inflections -mi, -si, -ti in the Egyptian letters of Arzawa, Miss Kober succeeded in 1946 in recognizing certain inflections in the Knossos tablets, but came to no conclusions as to the type of language she was dealing with. Georgiev, a Bulgarian scholar, went further, and postulated for 'Minoan' Linear B a pre-Hellenic Indo-European language which he labelled 'Pelasgian.' All this work was achieved before the appearance of the long awaited Pylos Linear B inscriptions of E. L. Bennett of Cincinnati and Yale (1951), and the Knossos Linear B inscriptions of Evans posthumously published by Sir John Myres in 1952.

Between 1950 and 1952 Michael Ventris recognized further evidence for two different sets of inflections which were labelled tentatively 'masculine' and 'feminine.' Having observed that Linear B signs invariably represent either vowels (say, a, e, i, o, u) or open syllables (say pa, pe, pi, po, pu), he realized that in the total vocabulary of Linear B the vowel initials would appear with a greater frequency than the consonantal. This observation led to the identification of the vowels, and five were in fact discovered. Ventris now applied Kober's 'grid' to the new Bennett-Myres material. Bennett had already made a similar analysis, but without, so far as is known, communicating his results to his European colleagues or publishing his results. Ventris, on the other hand, circulated his material among other scholars, including Bennett. Both men had made frequency counts on the fuller material now to hand. Enlisting the collaboration of John Chadwick for the final interpretation, Ventris made the first breach into the language. As one would expect, the word 'and' stood out clearly among words of similar inflection. Postulating a Greek, or at least an Indo-European type of language, this word was first read as PE (for classical τέ), subsequently as QE. Another clue was afforded by a word which occurred twice or more with one word intervening. This could

only be read as 'either . . . or,' 'neither . . . nor' or 'both . . . and.' The word has now been identified as *OUQE . . . OUQE . . .* 'neither . . . nor.' Meanwhile Cowley's recognition of the word for 'total' has been vindicated, and is now read as *TOSO*, 'so much.' Ideograms (*rebus* words without phonetic value) frequently betrayed the substance of the matter preceding them; a prehistoric form of Greek was read into the symbols, and the Mycenaean language once more emerged after an eclipse of some 3,000 years.

This method of decipherment, *viz.* the reading of Greek into a language antedating Homeric by some 300–700 years and written in a script apparently designed for a still older language ('Eteocretan'), is one which is fraught with risk, and the results must at this stage be interpreted with caution. As to the older language, the values discovered in Linear B ('Mycenaean') have been applied to the older Linear A inscriptions ('Eteocretan,' dated tentatively about 1500 B.C.), but the resulting jumble of words cannot be interpreted, and its links, if any, with other Mediterranean languages cannot be established. The Ibero-Caucasian theorists will no doubt study it with interest.

The Linear B, or Mycenaean, tablets are of two basic types, (a) page tablets ruled like old-fashioned school slates, and (b) tablets inscribed along the bottom edge, any residue being written above and towards the right of the tablet.

As a preliminary to decipherment a grammatical framework was set up. To do this in any detail with a syllabary as its basis is bound to lead to considerable disappointment, for as the characters can express nothing but open syllables final consonants cannot be represented, and the characteristic endings of the Greek and Indo-European noun (-s, -m or -n, -r, etc.) are not revealed: thus *I-QO* spells 'horse' (\**IQQOS*), *PA-TE* spells 'father' (\**PATER*). A more serious defect is that internal consonants cannot appear before other consonants, a mannerism reflecting the phonetic poverty of the older Eteocretan language: thus they are either suppressed as in *ATO-*, 'bread' (\**ARTOS*), or they are given a spurious vowel as in *TARANU*, 'stool' (\**THRĀNUS*). But greater knowledge of the values of -*A* and -*O* (here transcribed for convenience *Ā* and *Ō*) may throw more light on this problem. As in Hittite all the consonants with a similar point of articulation are represented by one character: thus *P* represents the consonantal triad *π*, *β* and *φ*, *K* does duty for *κ*, *γ* and *χ*, and *Q* reflects the three Indo-European labio-velars. But *D* (Greek *δ*) is distinguished from *T* (Greek *τ* and *θ*) by a separate sign, as is the iota-cized (*yot-affected*) reflex of IE \**gj-*, \**dj-* (Mycenaean *Z*, with the presumed value of *DZ*) from the reflex of IE \**tj-*, \**dhj-* (Mycenaean *S*). The separate *D* symbol may have been pressed into service from Eteocretan where it may have had a totally different value (cf. the Portuguese adaptation of Latin *x* to express *f*). The poverty of Eteocretan phonology is reflected further in the representation of IE \**l* and \**r*, for which there is apparently only one sign, thus *MORIWODO*, '(the metal) lead' (for \**MOLIGWDOS* or \**MOLIW DOS*, radical *mol-* 'heavy'). *S* occurs, as in Greek, when it is the reflex of some Indo-European consonant or consonants other than \**s*, indeed the presumed aspiration to which IE \**s* has degenerated is suppressed altogether. Consonant clusters can with few exceptions be expressed only by the syllabic echoing of a preceding or following vowel, thus giving the vowel a spurious double as in *KURUSO*, 'gold' (\**KHRUSOS*), *ATOROQO*, 'man' (\**ANTHROQOS*). Vowel length and diphthongs whose second element should be -*I* are not shown: thus *O* represents *o*, *ω*, *os*, *ov* (or *ou*), *oi*, *ø* and *œv*. The only diphthongs represented are *EU*, *AU* and *OU*, and perhaps the cluster *WJ*.

To offset these disadvantages Mycenaean displays (1) the *W* phoneme; (2) Indo-European \**ī* as in Doric; (3) intervocalic *yot*

(English *y* as in 'your'); and (4) the labial triad as distinct from the labio-velar (thus *P* as distinct from *Q*).

From the foregoing remarks it will be seen that Mycenaean, unlike Hittite and the 'barbaric' neighbour languages of ancient Greece, is a linéal ancestor of some form of Greek, indeed its compound words make this abundantly clear. Grammatically it stands close to Homeric, with the *O* stem genitive singular in -*OJO*, and the aorist without augment, as in *WIDE*, 'he saw.' The five cases include an instrumental, which though not demonstrable in the singular appears in the plural with the ending -*PI* (\**PHI*). One interesting fluctuation in vowel spelling arises in the representation of the Indo-European semivowels \**m* and \**n*. Words reflecting these two phonemes are written variously with *A* or *O*, thus *ENEWO*, 'nine' (\**ENNEWA*). This may indicate the existence of an intermediate vowel, perhaps a nasal. Privative *A-/AN-*, however, is always represented with *A*. Moreover the existence of two signs for *RA* coincides with the fact that Greek *λα/ρα* have each two values: (a) IE \**la/ra* and (b) IE \**l/r*. Neuter plurals of Greek type in -*α* are represented by -*A*, those of type -*ει* (invariably at the mainland sites) by -*EA*. Laryngeal theorists will be interested in this.

The inadequacy of Mycenaean spelling precludes for the moment any pronouncement as to whether the language preserved the palatal series *k'*, *g'*, *g'h*, known as the *satem* triad. *AKETA*, 'leader' (*ἀρχης*), and *WOIKO*, 'house' (*οἶκος*), do not display palatalism.

As one would expect, a language of this degree of antiquity contains many words unknown to Greek. To try to force them into an Indo-European framework now would be premature, indeed the prevailing uncertainty about phonetic values, the ambiguity of the spelling, and the uncertainty as to meaning, added to the possibility that some Mycenaean words have been borrowed from Eteocretan, are factors which serve to underline the need for caution.

#### SKELETON PHONOLOGY

(Supplied phonemes are bracketed; spurious vowels are underlined; *A* and *O* are represented *Ā* and *Ō*)

IE

*p* *PATE(R)*, 'father'; *RAPTE*, 'tailor.'

*b* —

*bh* -*PI*, instrumental plural inflection.

*t* *TOSO*, *TOSA*, 'so much'; *TOTO* (or \**TOITO?*), 'this'; *TIRI*, 'three'; iota-cized in *TOSO* (from *TOTJO-*); before *i* in *PĀSI-REW*, lit. 'Statt-halter' (*βασιλέως*) as distinct from *WANAK-*, 'king' (*ἄνωξ*); before *ū* in *SUZA*, 'figs.'

*d* *DUWOU-*, 'two'; *DOSE(I)*, 3 sg. 'will give'; -*PODE(S)*, 'feet'; iota-cized in *TO(R)PEZA*, 'table'; *WIRIZA* (queried), 'root.'

*dh* *TARANU*, 'stool' (Hom. *θρῖνυς*, cf. Skt. *dhi-*, 'support').

*k'* *KARA*, 'head' (Skt. *śiras*, etc.); *KUN-*, 'dog'; -*KEREWE* (in names), '-famous' (-*ληνς*); *WOIKO*, 'house' (*οἶκος*, cf. perhaps Latin *uxor* for (?) *woik-sōr*, 'housewife').

*g'* -*AKETA*, 'leader'; iota-cized in *WO(R)ZE(I)*, 'does' (Arm. *gorčé*, *id.*, cf. *εργον*); *MEZO*, 'bigger' (cf. Arm. *meč*, 'big,' and Gk. *μεγάλων*).

*g'h* *WOKAWEJEKEE* (epithet of wheels) (cf. Gk. *δχος*, 'car'); *EUKETO(I)*, 'declares' (εὐχετεῖται; Skt. *śahate*); *EKO*, 'have' (Gk. *ἔχω*; Skt. *sāhē*, 'subdue,' cf. Alb. *gjedh*, 'cattle').

*k* -*KORO* in *QOU-KORO*, 'herdsman' (*βουκόλος*).

*g* *ZE(UKO)* (queried), 'pair, yoke'; iota-cized to *Z* as under 'g' above.

- gh* -*KI*, sentence particle in *OUKI* (Hom. οὐχι: Lith. *ui-gi*, *taipo-gi*, etc.: Slav *ži*: OE *ge*, OHG and Olcel. *-gi*, Arm. *g-* in *g-čči*).
- gw* QETORO-, 'four': QERANA, 'jug' or 'pot' (Olcel. *hverna*, [Feist], beside *hverr*, 'pot': Skt. *čaruh*: Ir. *coire* (whence 'corrie'), cf. Hittite *is-qaruh*, 'a vessel,' and Lat. *cōr-tina* (a compound), cited by Thurneysen and needlessly rejected by Hofmann, the vowel as in *sōptō*, *sōdēs*, *cōnor* [=πνωραι], etc.).
- gw* QOU-, 'ox'; before a consonant MORIWODO, 'lead' (μόλιβδος, radical *mol-*, 'heavy,' cf. χάλ-υψ, 'steel,' radical *ghal-*, 'hard,' as in χαλεπός).
- gwh* QE(R)TEA, 'debts' (τίλθεα).
- jl* REWO(N)T-, 'lion' (λεον-τ-, lit. 'seizer,' cf. βασι-λεύς and Slav. *loviti*, 'seize, hunt').
- r* KO(R)WO, 'boy' (κοῦρος).
- m* MEWIJO or MEUJO, 'less' (*uelow*, cf. Olcel. *mjör*, f: *mjó*, 'slim' — Holthausen).
- n* NEWO, 'new' (*vtoś*); WIRINIVO, 'of leather' (cf. Gk. *pívōs*, 'skin').
- s* Lost, except as part of certain clusters, see below.  
(1) ZE(UKO) (queried), 'pair'; (2) WOJO, 'own' (gen. sg. Hom. οὐ, cf. Slav *svoj*, 'own,' Skt. invar. *svayám*, 'self').
- w* ETEWO, 'true' (Ἐτέος); WETO, 'year' (ἔτος); lost initially before R as in RAPTE, 'tailor,' and between A and O as in POSEDAONE, dat. sg. 'to Poseidon,' perhaps also after N, cf. ENEKA, 'for the sake of.'
- a* WA(S)TU, 'city'; WANAK-, 'king'; AROURA, 'ploughed land'; ARARUJA, f. 'having (been) fitted' or similar.
- ā* TARANU, 'stool'; DAMO, 'people'; KARAWE(S), 'old women' (γρνής); KATI, 'box, urn or basket' (κῆθις, cf. Hes. κάθος, 'basket').
- c* -KEREWE, '-famous'; REWO(N)T-, 'lion'; WE(R)WE-, 'woollen' (ἴποιος, cf. Hitt. *warhuis*, gen. sg. *warhwayas*, 'shaggy, unshorn'); QE(R)TO (queried), 'pot,' etc.
- i* IJEREJA, 'priestess' (note absence of W implied in the masc. Greek form *Ιεπεύς*).
- t* (WIRINIVO, 'of leather,' may derive from a radical \*wrism-, hence not historically long).
- o* -PODE(S), 'feet'; TOQO (queried), 'place' (τόπος, cf. further Slav *toliti*, 'turn,' R. *tókarī*, 'turner,' with semantics of πόλος: Slav *kolo*, 'wheel'). Unreduced o occurs in PARO (παρά), QETORO- (τετρα-) and EUKETO(I) (εὐχετα).
- ō* DOSE(I), 'will give'; òw in QOU-, 'ox,' DUWOU-, 'two,' and perhaps in -OWE(S), '-ears.'
- u* TURÓ, 'cheese.'
- ū* SUZA, 'figs' (cf. σῦκα).
- shva* TATOMO (=σταθμός).
- ! WARAWITA, adj. neut. pl. (epithet of wheels, cf. Gk. ἄλις, 'roll').
- PARO, 'from the midst of' (παρά: Goth. *fair*); RAPTE, 'tailor' (cf. βόπτω: Lith. *virpti*).
- m* ENEWO, 'nine'; PÁSIREW, 'governor.'
- n* A-, AN-, privative prefix; perhaps also ÁTERO, 'other of two.'
- au* TAURO, 'bull.'
- eu* EUKETO(I), 'declares.'
- ou* AROURA, 'ploughed land' (ἀρουρα).
- ai* PARAJA, neut. pl. adj. (queried), 'old' (παλαιός).
- ei* E(I), dat. sg. of i stems.
- OI* WOIKO, 'house'; -O(IH)I, dat. pl. of o stems.
- Clusters: KITIE(N)SI (κτιζούσι); QISIPEE, 'two spears or thrusting swords' (cf. ξίφος); IQO, 'horse' (ἵππος);

AIKASAMA, 'spear point' (αἰχμή, cf. Lith. *iešmas*, 'spit,' IE \*aiksm- as λύχνος from \*lukne-). Grassmann's law of dissimilation in KATI (κηθίς).

There is, of course, nothing final about these interpretations, and the spelling may have to be modified.

#### SKELETON GRAMMAR

##### Nouns

O masculines: KO(R)WO 'boy'; WOIKO, 'house'; A(N)TO-ROQO, 'man'; QOUKORO, 'herdsman'; gen. sing. -OJO; dat. pl. -O(IH)I. N.B. O masculines have no instrumental plural in -PI, at least at Pylos.

O neuters: KORIJADONO, pl. KORIJADONA, 'coriander' (κοριαννόν); KUMINO, pl. KUMINA, 'caraway' (κούμινον). A feminines: AROURA, 'ploughed land'; WIRIZA (queried), 'root.'

Consonantal: REWO(N)T-, 'lion'; inst. pl. REWO(M)PI; -PODE(S), 'feet'; inst. pl. -PO(D)PI; PATE(R); WANAK-; PÁSIREWIJO(N)TE(S) (queried nom. pl.), 'ruling,' cf. Hittite *kurtimiyawanea*, 'being angry.'

I declension: No certain evidence for oblique cases, cf. nom. sg. APUDOSI (ἀπόδοσις).

U declension (nouns of function): TARANU, 'stool'; nom. pl. TARANUWE(S); PÁSIREW, gen. sg. PÁSIREWO; feminines: KARAWE(S), 'old women'; neuter: WA(S)TU, 'city' (δστυ).

OS neuters: WETO, 'year'; PA(R)WO, 'cloth' (φάρος); QE(R)TO, 'debt'; gen. sg. -EO; nom. pl. -EA.

Anomalous: DIWE, dat. sg. 'to Zeus' (Διῖ).

##### Adjectives

Primary: TOSO, fem.: TOSA, 'so much, so many'; NEWO, 'new'; WISQOWO, 'equal' (ἴσος, cf. Skt. *viśvam*, acc., equal; equinox).

Derivative: KOMAWE(NS) (queried), 'long-haired' (cf. κόμη); -KEREWE, '-famed'; WARAWITA, neut. pl.; WANA-KÁTERO, 'royal.'

Comparative: MEZO (μεζών), MEWIJO or MEUJO (μελών).

##### Numerals

1, -E (εἰς, ἵν); 2, DUWOU-PI, 'duobus'; 3, TIRI-; 4, QETORO-; 6, WE(KS); 9, ENEWO.

##### Verbs

Primary athematic: PASI, 'says' (φησι); DIDO(N)SI, 'they give' (διδοῦσι).

Primary thematic: WO(R)ZE(I), 'does, makes'; EKO, 1 sg. 'have.'

Future: DOSE(I), 3 sg. 'will give'; DOSO(N)SI, 3 pl. id.

Aorist: OPORO(N), 3 pl. 'owed' (δεχθον); WIDE, 3 sg. 'saw' (Hom. θε).

Middle: EUKETO(I), 3 sg. pres. 'declares.'

Participles. Present: PÁSIREWIJO(N)TE(S), nom. pl. (βασιλεύοντες), EKO(N)TE(S) (ἐκοντες); future: DEMEO(N)-TE(S), nom. pl. 'for building'; perfect: TETUKWOA, m. (cf. -os), ARARUJA, f. (-ωια), cf. Arm. *arar*, 'doing, deed'; imp. TORQEJOMENO, 'turning' (τροπεύενος); WO(R)-ZOMÉNO, 'done, made' (cf. Hom. *τοργυμένος*, Arm. *gorcem*, 'I do, make,' and the Arm. verbal nouns in -um).

##### Pronouns

TOTO (or TO(I)TO?), 'this'; WOJO, 'own' (gen. sg. Hom. οὐ).

*Prepositions*

PARO (with dat.) 'from among' (cf. παρά in its genitive usage); ENEKA, 'for the sake of.' Postposition: -DE (-ει).

*Particles*

OU-KI, 'not' (Hom. οὐχί); DE (δε).

Adjectives and short genitive constructions precede the nominative as in Lithuanian. There is apparently no definite article. The use of sentence particles resembles Greek and Hittite practice.

*Etymology*

Despite the archaism of its phonology and vocabulary Mycenaean is full of pitfalls and ambiguities, and linguistic interpretation must proceed with caution. So far the type of verb represented by Gk. ποίεω (cf. Skr. *kavayāmi*, 'compose'; Alb. *sajoj*, 'shape') has not emerged; instead we have radicals *WO(R)Z-*, *AR-* and some evidence of a type *QE(R)-*, *QO(R)-* (Skr. *kr-*). Many Mycenaean words are obsolete in Classical Greek, and these will engage the attention of comparatists for some time to come. Despite the ambiguities of the spelling the labio-velars and the *W* phoneme emerge clearly and make Mycenaean of inestimable value as a corrective. In the light of the new discoveries the existing etymologies of *ένεκα*, *έννια*, *τρέπεσθαι*, *μελών* and *λέπεσθαι* will have to be revised, but the history of *Ιττός* and *οὐ* remain as problematical as ever. For the rest Mycenaean will in course of time supply the answer to many of our urgent queries. Already many of the words dismissed by Boisacq as 'Mediterranean' or 'of unknown etymology' have turned out to be Greek compounds, others will no doubt come clear in the light of the new evidence. Meanwhile we await with interest the appearance of the work of Ventris and Chadwick some time this year. By the courtesy of Ventris I have been given a preliminary sight of their joint material, and in view of their achievement it is a pleasure to welcome them to the growing ranks of serious comparative linguists, who are all too few in this country. Digging at Knossos and elsewhere continues, and if, as is hoped, consecutive texts are ultimately found, we shall be abundantly rewarded.

**Trade-Wind Beads.** By Dr. W. G. N. van der Sleen, Amsterdam.  
With three text figures

27

In some European museums one may find a few beads, collected on the beaches of Zanzibar, Kilwa or Pemba. Little is known about these beads and yet they may be of great importance to the history of the east coast of Africa and of the trade routes in the Indian Ocean.

After collecting and studying beads on a caravan trip from the Cape to Lake Kivu in the Belgian Congo, including visits to Zimbabwe, Khami, Mapungubwe and other famous ruin sites, I was lucky enough to be able to visit the beautiful Zanzibar beaches. I was able to collect a quantity of beads there which struck me at once as being totally different from everything I had before seen in Africa south of the Equator. The museums in Zanzibar, Dar es Salaam and Gedi (80 miles north of Mombasa) had series of similar beads on show.

These beads can be classified into six groups:

- I. Beads made from semi-precious stones, e.g. agate, carnelian, quartz, etc.
- II. Indian-red opaque glass beads.
- III. Biconical beads, mostly lentiform, made of the same opaque glass.
- IV. Spherical standard beads wound from thin rods of glass.
- V. Rough cylinders and oblates of striking colours, yellow, green and blue.
- VI. Small beads, 2-5 millimetres in length and/or diameter.

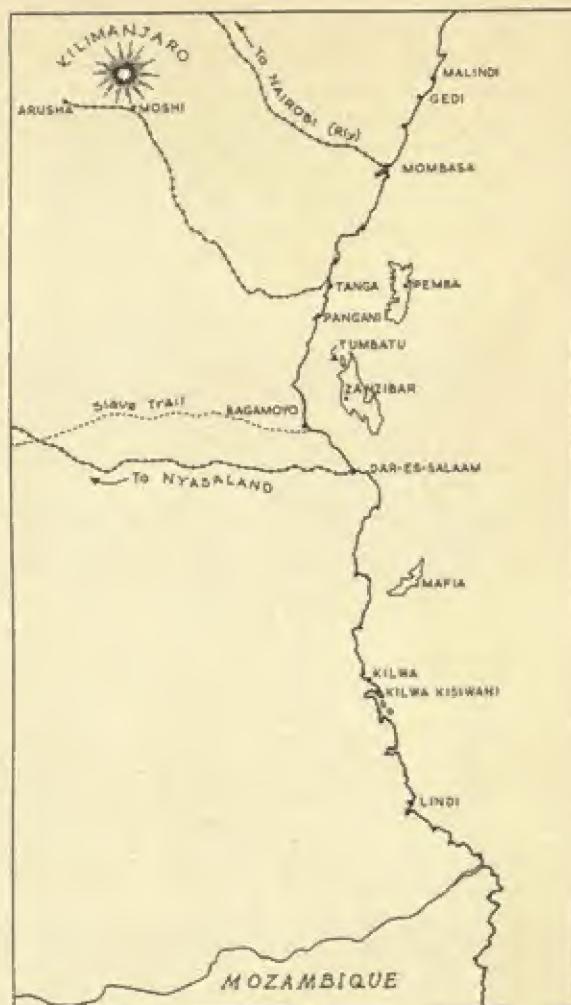


FIG. 1. EAST COAST OF AFRICA

They differ absolutely from modern and Egyptian beads as well as from the beads of the Great Migration (c. A.D. 600) in Europe. Perhaps more important is that they occur not only in East Africa at the above-named places, but are also identical with beads found in stone slab graves from Sumatra and Java, and also with the beads described by Beck in MAN, 1930, 134, from Coimbatore, Kuala Selinsing, etc., and mentioned from Tangal by the same author in Dr. Caton-Thompson's book, *Zimbabwe Culture*. Beads have been found in Zimbabwe as well as in Mapungubwe that fit in very well in the East African series. All these beads, if dated, have been used in the third to the fourteenth centuries A.D. They are all pre-Portuguese, often much older. Professor Millot in Antananarivo, G. Hunter in Dar es Salaam and J. Kirkman in Gedi date them between A.D. 1200 and 1400. H. C. Beck dates his finds about A.D. 800 and van der Hoop goes back still further to the third century of our era.

The reader will now understand why I want these beads to be called Trade-wind beads, as I am sure they must, next to the cowrie shells, have formed the small currency of the Chinese junks and Arab dhows that traded their goods from China to Ceylon and from Ceylon down south to Sofala and Madagascar.

I will now describe the six groups of beads.

*I. Beads made from semi-precious stones*

These beads might be called the most beautiful of the Trade-wind beads. I have a dozen carnelian or agate ones, two made of

onyx from Jaipur, three of quartz, and one of garnet (fig. 2a, b). Two of these carnelian beads are hexagonal barrels, one inch long, a form that does not occur as far as I know before the beginning of our era; the others are very different as has already been shown by Dr. A. J. Arkell in *Antiquity*, Sept., 1936, 'Cambay and the Bead Trade,' where he points out that Cambay may be the source of nearly all the carnelian and agate beads that are found all over Africa, in Southern India, Indonesia and China. The old historian De Barros writes, that when the Portuguese

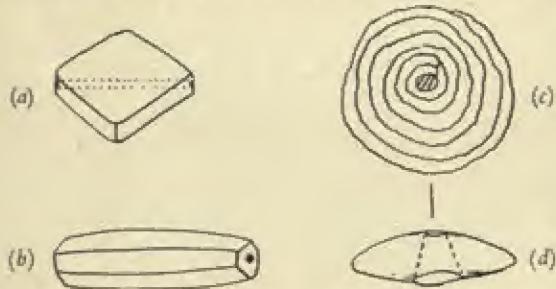


FIG. 2. (a, b) CARNELIAN BEADS, GROUP I ( $\frac{1}{1}$ )  
(c, d) 'GUIDE BEAD,' GROUP III ( $\frac{2}{1}$ )

first arrived in Kilwa (Quiloa), they found ships from Cambay in the harbour. Probably the onyx and quartz beads came from the same source.

Other forms of carnelian Cambay beads are lozenges, 20 millimetres long and 12 millimetres wide. Six of this type were found by Hall in Zimbabwe and are now in the Cape Town Museum. Others are found with great quantities of cruder forms on the beaches of Natal and Zululand. These probably formed the load of a Portuguese galleon that was shipwrecked on the coast.

### II. Indian-red opaque glass beads

These are the most numerous of the Trade-wind beads. Under the microscope, small chips of these beads show that the glass is greyish and transparent, but contains innumerable small copper crystals, which give it the opaque red colour. The beads are very variable in form and size, ranging in shape from cylinders to oblates and rings, and in length from 4 centimetres to 2 millimetres and in diameter from 1 centimetre to 2 millimetres. Nearly all are drawn or cane beads, very few are wound, and these last are found in Zanzibar as well as in Mapungubwe.

We may summarize our knowledge of the Arab, Portuguese and Dutch bead trade in a few words: 'beads of Cambay, red coral and clay beads' and 'green, yellow and blue glass beads from Negapatam.' Now the real red coral beads from the Mediterranean are very scarce along the African coast. The clay beads (*barros muidas*) of the Portuguese writers can have been none other than our Indian-red glass beads, that were described by many modern authors as 'paste,' because it looks so unlike glass. They are found in great quantities in all East African Arab and pre-Portuguese sites and even much further inland, in the Rhodesias and the Transvaal (see, e.g., the Bavenda heirloom beads). When the Portuguese first arrived at the East African ports, the natives would not accept the beads brought from Europe, so that one of the Portuguese kings had to write to his Governor in Negapatam, to ensure that his ships could be provided there with the beads wanted by the natives on the African coast. This is of course no proof that these beads were made in Negapatam.

### III. Biconical beads, mostly lentiform, made from opaque glass

These are made in various colours, viz., Indian-red, black, green and yellow and all with a conical perforation. This last

character distinguishes these beads from the lentiform or disciform beads of the XVIII and XX Dynasties in Egypt, nearly all of which have a cylindrical perforation. Another difference is that the East African beads are wound from very thin rods of glass, which is not the case with the Egyptian beads (figs. 2c, 3a).

This form of bead is rare. I collected about two dozen specimens only on the Zanzibar beaches; even so, I should like to consider them as a 'Guide Bead' just as we know 'Guide Fossils' in geology. In India and Indonesia they are found in much smaller numbers, but as soon as a few hundred beads are available, one or two of the disciforms will occur. The real guide-type is 3–4 millimetres long and has a diameter of 8–12 millimetres. This group, together with Group IV, must be looked for very carefully and may lead us to their place of manufacture.

I have half a dozen long biconical beads from the same site and of the same make and material.

### IV. Standard spherical beads, wound from thin rods of glass

These beads are globular or oblate and have diameters of 12–8 millimetres. Like the biconical beads, they have not been heated sufficiently to make the surface melt evenly, and still show the inequalities caused by the winding of the small rods (fig. 3b). They are often shiny black, but I found green, blue and yellowish ones too. One of these beads is illustrated in the colour plate of Dr. Caton-Thompson's book. There is a string of 25 to 30 of them in the Salisbury Museum.

### V. Rough cylinders and oblates, yellow, green and blue

These beads, together with the Indian-red beads, form the majority of the Trade-wind beads. The colours are quite different from European and African beads. Pure green or blue seldom occur, but there are many shades of blue-green and greenish blue. Again, nearly all these beads are drawn, and only a few are wound. They must be the beads of green, yellow and



FIG. 3. (a) BICONICAL BEADS, GROUP III ( $\frac{1}{1}$ )  
(b) SPHERICAL BEADS, GROUP IV ( $\frac{1}{1}$ )

blue glass from Negapatam that I mentioned before under Group II. These same beads were described by Beck from Asiatic sites and found by me in collections from graves in Java and Sumatra. They are quite different from the beads found in India by Sir Mortimer Wheeler in Arikamedu and in the so-called Indus Civilization, which are dated to the first centuries A.D.

### VI. Small beads, 2–5 millimetres in length and/or diameter

This group of beads resembles the finds at Mapungubwe, Zimbabwe and other Rhodesian ruins, which are generally very small and regular and often occur in large numbers, up to thousands and tens of thousands in one grave or level. Amongst these so-called 'seed-beads' we find however a varying number of somewhat larger and more irregular beads. Beads measuring more than 5 millimetres are rare in these ruins, but the Trade-wind beads are generally larger. In the case of the larger beads the colour and manufacture may tell us something about their origin, but the smaller specimens will have to await spectro-

scopical analysis as trace elements may play a role in recognizing glass from different periods or factory sites. To date, chemical analysis gives no clues. There is no marked difference between old Egyptian, Roman, Venetian or Indian glass. I think that most of the beads from the East African ruins will some day prove to have come from mediæval Venice, but that a certain percentage will have come from some forgotten factories along the coast of the Indian Ocean.

We will have to look for these factories, at the same time collecting notes and material from all points of the Indian Ocean between China and Madagascar. This study may bring us to an answer to the greatest riddle of southern Africa: who brought the Trade-wind beads so far south and so far inland and who were the people who supervised the building of Zimbabwe, the terraces of Inyanga and other ruins? When we look not only at beads, but also study architecture, pottery and ceramics, inscriptions, etc., our thoughts will turn to Persia. History tells us that Kilwa was originally built in the tenth century A.D. by people who were driven out of Oman on the Persian Gulf at the end of the seventh century and travelled slowly further and further south. We find among the titles of Khalif bin Harub: Sultan of Oman, Musquat and Zanzibar. Shiraz in Persia is often named as the original home of the Zanzibarians and some people speak of a Shirazian architecture. It is of interest too, that in the twelfth century a Venetian glassworker settled in Shiraz, to teach people there how to make better and more beautiful glass than they made before.

**Structures de parenté et d'alliance d'après les formules Pende.** By L. de Sousbergh. Acad. roy. des Sci. colon., 28 Mém. Vol. IV, Part 1 (Ethnog.). Brussels, 1955. Pp. 92, 1 illus., map. Prize Belg. fr. 120

One of the most remarkable works in the literature on kinship is Lévi-Strauss's *Les structures élémentaires de la parenté*, but a disappointing feature of its reception is that the value of its analyses has not been tested by ethnographers in the field. The present monograph is therefore of greater importance than it might otherwise appear, for de Sousbergh studied Lévi-Strauss's themes in the field and has criticized them specifically with reference to his own original ethnographical material. No one who is interested in structural theory should miss this ethnographically rather unsatisfying work.

The people studied are the Pende of the southern Belgian Congo. The author, a priest, spent two years (1951–1953) among them and evidently understands them and their language well. They are a matrilineal people with patrilocal marriage, politically divided into chiefdoms. Over the greater part of Pende country the preferred marriage is with the father's sister's daughter, but where a man cannot get a wife from his father's clan he can turn for one to his mother's brother. In one area mother's brother's daughter marriage is forbidden, and the alternative source of a wife is the mother's father's clan.

De Sousbergh rejects Lévi-Strauss's notions concerning father's sister's daughter marriage since they are not schematically represented in Pende thought, and claims that the opposed type of exchange—*échange généralisé*, with mother's brother's daughter marriage—has nowhere been described in its circulating form and is unlikely to exist. On the basis of Pende concepts about their society he denies the value of Lévi-Strauss's analysis in terms of exchange and ignores the structural implications of the different types of cross-cousin marriage. In their place he proposes to explain cross-cousin marriage by reference to Rattray's Ashanti hypothesis and his own concept of 'reunion'—'the exact opposite of a formula of exchange.' Cross-cousin marriage is preferred, he believes, be-

I am therefore somewhat inclined to talk about Persian instead of Arab influence in these regions, although it may have been Arabs who manned the dhows that brought the Sons of Sindbad to these foreign shores. When we know a bit more about all this, we will have to ask if the ruins of Zimbabwe too were built by the Bantu under the fear-inspiring eye of the Persians, and if the same Persians pressed the Bantu to work in the gold mines of Monomotapa, and if the terraces of Inyanga and so many other fantastic works were all brought into being by the descendants of Darius and the followers of Zoroaster!

#### Note

The author would be glad to hear from correspondents in the areas of East Africa and the Indian Ocean who may be able to help in this research.

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## REVIEWS

### AFRICA

cause a marital alliance reunites in a subsequent generation what alliances in preceding generations have separated. It serves to 'réunir par des alliances préférentielles, dans les générations suivantes, les éléments d'une même descendance séparés par la loi de prohibition de l'inceste. Cette tendance est à base sinon de culte, du moins de vénération ancestrale ou tout au moins d'une conscience aiguë de solidarité ancestrale; . . . Elle [l'union préférentielle] contraint la prohibition absolue de l'union frère-sœur, par l'union préférentielle de leurs enfants.' But he has to admit that this does not explain why parallel cousins are not preferred mates or are prohibited. He concludes by enunciating certain propositions about the problems that Lévi-Strauss deals with, and attacking in rather spleenetic manner the positions which the latter adopts.

Dr. Luc de Heusch has written an admirable critical commentary on de Sousbergh's monograph (*Zaire*, Vol. IX (8), 1955, pp. 849–861). His main points are a demonstration (from de Sousbergh's material) that in fact the Pende are not as indifferent to the distinction between marriage with the father's sister's daughter and with the mother's brother's daughter as de Sousbergh claims; that the Pende conception of the circulation of the 'semence' of men does not contradict an analysis in terms of circulation of women; and that de Sousbergh pays overmuch attention to indigenous ideas, which alone he apparently conceives as sociologically valid. These fundamental criticisms make it impossible to accept de Sousbergh's argument; but there is more to be said than even this excellent commentary, and I hope to devote an article to doing so. For the moment I cannot do better than urgently recommend readers of de Sousbergh's monograph not to fail to read de Heusch also.

De Sousbergh's central difficulty seems to be a refusal to countenance morally neutral structural facts, and a consequent incapacity to entertain their analytical implications. It is a very great pity that such an obviously promising ethnographical field as the Pende, and such knowledge of the people as de Sousbergh possesses, should not after all have provided anything like an adequate test of the elucidatory value of Lévi-Strauss's challenging theories.

RODNEY NEEDHAM

**Nineteenth-Century Borneo: A Study in Diplomatic Rivalry.**

**29** By Graham Irwin. *Verhand. v. h. Koninkl. Inst. voor Taal-, Land- en Volkenkunde, Publ. XV.* 's-Gravenhage (Nijhoff), 1955. Pp. xii, 251, maps. Price 10 guilders

There is no social anthropologist who is not interested in the history of the people or area he studies or who would decline to use historical evidence about them. The trouble is, of course, that in the main the parts of the world that anthropologists have traditionally studied have no history, and professional historians have not concerned themselves with them.

The internal history of Borneo has been dealt with by various authors, but for very restricted areas and periods. Their studies have also suffered from the old and crippling defect of Borneo scholarship in that they have dealt with only one or other of the two major political territories of the island, as described in either English or Dutch sources.

The present work is by a lecturer in the Department of History of the University of Malaya, and is the first general account of its kind of a period of Bornean history. It covers an important part of the last century (1809-1888), is based on material from both English and Dutch sources, and deals with the whole of Borneo. A political study of the diplomatic rivalry between England and Holland, it is not concerned with anthropological issues (the author's purpose is 'to view the history of the island not from Bandjermasin or Kuching, but from Batavia, London, and The Hague'); but the events it describes affected the ideas and economies of the indigenous peoples, included their first major experiences of Western expansion, and shaped their political allegiances today.

It is not for a non-historian to attempt a detailed critique of a historical work, but he can appreciate the very scholarly care of its construction and the minutely conscientious detailing of sources. An anthropologist might make the minor comments that there is in fact no evidence that any of the tribes of North Borneo are 'part-Chinese in origin' (though there has been considerable intermarriage in historical times), and that there is as little that Robert Burns was 'predatory' on Kayan women (he would have lost his head if he had tried to be). A more general and important point is the possibility that sometimes too great reliance is placed on the statements of Malay rulers as evidence of intent or disposition. These potentates were responsible to no one but themselves, and their words did not have to conform to declared policy. Also, official utterances by Malays are marked, in general, by an optimism and a generosity of sentiment which would be highly unusual in European statesmen. How far one bases one's actions upon them depends on how well one understands Malay culture. Brooke and Raffles were good judges: it is more difficult for the historian accustomed to the political systems and diplomatic language of Europe.

The book is well produced, with clear regional maps; but there should also be a map showing the whole of Borneo. The bibliography and index are full and well ordered; and there is also an extremely useful appendix, a practical guide to Dutch colonial records in the *Algemeen Rijksarchief*.

Dr. Irwin's account is readable and scholarly, a pioneer study in a new approach to the history of Borneo. It should be in the library of anyone interested in this part of the world.

RODNEY NEEDHAM

**Iban Agriculture. A report on the shifting cultivation of hill rice by the Iban of Sarawak.** By J. D. Freeman. Foreword by E. R. Leach. *Col. Res. Stud.* No. 18, London (H.M.S.O.), 1955. Pp. xii, 148, 24 plates, 5 text figs. Price 12s. 6d.

This is a report on one of the social-economic research projects in Sarawak recommended by Dr. E. R. Leach in 1948. The author's assignment was 'a study of a traditionally based, stable Iban community based on shifting dry rice cultivation, and not subject to undue land shortage.' As a landmark in anthropological research on agriculture it stands beside Dr. Bohannan's masterly report on another type of agricultural economy (*Col. Res. Stud.* No. 15), and should be compared with the relevant parts of Dr. Geddes' study of the Land Dayak (*Col. Res. Stud.* No. 14). In contrast to previous

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reports on Sarawak in the same series it is very well produced in book form, and cheap.

Dr. Freeman had unusual, almost ideal, advantages in his research. He was given funds and facilities for two and a half years of field-work (1949-51), he worked through a language for which there are good grammars and dictionaries, and he studied a people who have attracted literary attention for a hundred years. Moreover, he found the Iban personally congenial, and by report was quite capable of dealing with their truculence on their own terms. It is quite clear from this first product of his research that he made excellent use of these circumstances.

The Iban are a people numbering over 190,000 in Sarawak. They belong culturally to the south Borneo grouping of tribes. At the beginning of the nineteenth century there were probably no Iban living north of the southern watershed of the Rejang basin, but they now inhabit the Rejang and its tributaries up to the Belaga and Baleh rivers and are present in substantial numbers in every administrative division of Sarawak. This distribution is the result of rapid and powerful invasion of the sparsely inhabited Rejang area, an advance which was checked by conflict with the southerly and westerly movements of middle Borneo tribes and by the Brooke administration. The aim and means of this most important ethnic movement was the shifting cultivation of dry rice. It is this which is the subject of Dr. Freeman's admirable and completely commendable monograph.

The investigation was concentrated mainly on the Baleh region. The results are presented in six parts: a summary introduction to Iban social organization (with the very necessary reiteration that the social and economic unit is the 'bilek-family,' not the allegedly cooperative longhouse group), then sections on land tenure (including history), agriculture, the economics of agriculture, Iban methods of land usage, and finally a shorter section on the problem of shifting cultivation with the author's practical recommendations. The stages of the description are illustrated by photographs, and the data and conclusions set out in meticulous tables. Iban religious objections to the surveying of their farmlands prevented the making of maps (such as provoke admiration of Bohannan's report). The resulting lack of precise acreages is a limitation to the value of the report and constantly hampered Dr. Freeman in his research; but it is evident that Iban obduracy was not to be overcome or their ban circumvented in any effective (let alone moral) way.

The author condemns certain types of Iban land usage, particularly that of sowing one field two seasons running, and Iban wastefulness of land and resources. He comes to the following conclusions: under Sarawak conditions, if land is worked for one season only, and then allowed to recuperate, adequate regeneration takes place; the cultivation-cycle should be about 12-15 years; land used in this way may be cultivated almost indefinitely without serious degradation taking place, and shifting cultivation is then an economically justifiable process. A caution must be repeated from Dr. Leach's foreword, that this is a study of extreme conditions, with waste of abundant resources, and that capital waste is not a necessary feature of shifting cultivation.

A few comments. There seems no need for the constant use of the word *padi* instead of 'rice,' or for 'bilek-family' instead of 'family' (once this term has been defined in the Iban context). When Kayan and Kenyah women sow rice the range of seeds in the dibble-hole is similar to that of the Iban (e.g. 3-17 on one Kayan farm), but very often seeds are scattered outside the hole as well. These do not reach maturity, and part of the seed is thus lost, but in this report no account is taken of such normal wastage. The germination-rate of the seed is also an important element in calculating return from labour and 'capital.' An appendix by a plant physiologist on the varying rates for different kinds of seeds, types of soil, and climatic conditions would have been valuable. The possibility of chemical fertilizers is not mentioned. Would they be quite irreconcilable with Iban religious beliefs? It would also have been interesting to learn more about the economic effects of beliefs in omens and other ritual prohibitions. Provinse, for instance, in his study of the Siang of the upper Barito, found

that no work was ever abandoned or left undone except for reasons of utility. It is surprising, incidentally, that Freeman makes no reference to Provinse's study of shifting cultivation by another Bornean people. The Siang material was based on a very much shorter study and can hardly be compared with Freeman's own, but it is not negligible. Cf. Provinse on the equivalence of a man's and a woman's labour, the distinction between exchange labour and paid labour, or—more important in comparison with the Iban practice which meets with Freeman's strongest condemnation—the fact that Siang only rarely plant rice in any given field in two successive years.

Finally, a protest against the horrid neologisms of 'utrolateral' (try saying it quickly) and 'urolocal.' Firth and Leach have used the term 'ambilateral' for descent systems in which at any point in a genealogy kinship can be traced through either side; and, consistently, 'ambilocal' refers to marital residence in either the husband's or the wife's group. There seems no good reason to abandon these satisfactory terms, especially when their retention may banish the ambiguity of 'bilateral' and 'bilocal.'

The general ethnographic importance of this book is that it is the first intensive social anthropological account of the Iban, and introduces—as the author promises—further works on Iban social structure and religion. Dr. Freeman must also possess a wide knowledge of peoples peripheral to the Iban, and could do much to clear up the chaos of upper Rejang ethnography. His book supplements the work of Geddes on the Land Dayak, Morris on the Melanau, and T'ien on the Chinese of Sarawak. A study of the nomadic Penan of the interior has been made, and another on a North Borneo people begun. When the results of these researches (as well as of other investigations into health, diet, and Kelabit culture) are published we should at last have a corpus of sound professional work dealing with the characteristics of Bornean social organization (and ambilineal systems in general) and the chief types of economy, representative of the three major cultural areas of Borneo. It is unlikely, however, that any future account will surpass Dr. Freeman's as a model of ethnographic thoroughness.

RODNEY NEEDHAM

**The Arts of Malaya.** By Tony Beamish. Singapore (Donald Moore) (U.K. agents: U. of London P.), 1954. Pp. 80. Price 7s. 6d.

**31** Culture is so rampant now in Indonesia and Malaya that a popular pamphlet on the arts of the latter is appropriate to the age, and if instead of running deep it froths with enthusiasm, it is typical of its time and place. The author deserves credit for attempting to cover the whole field. Malay, Chinese, Indian and European, painting, sculpture (which is non-existent!), architecture, music, drama, metal-work and pottery. It is, however, doubtful if he serves Malaya well by such exaggeration as the suggestion that 'the full range of Chinese art' has had any effect at all on the culture of Malaya except that the local Chinese have stuck partly to their homeland's architecture and dress and will quote Confucius from an English translation. The author is so anxious to collect grist for his mill that (unlike the Education Committee headed by Sir Alexander Carr-Saunders) he has a good word for Singapore architecture and can commend what used to be nicknamed the more-or-less style once favoured by the P.W.D. at Kuala Lumpur. In accordance with historical precedent and present political aspirations, he envisages a synthesis that shall beget a new art. But oddly enough he overlooks the most salient instance, the Malay adoption of the patterns of Indian textiles. And

like the politicians, he overlooks racial pride and prejudice, religious tabus and a climate 'where it is always afternoon.' The one art form that fascinates all races is the cinema, and evidence of the culture of the man in Malaya's streets is the remark of a Straits-born Chinese that she failed to find what she wanted in London and had to look for it in Blackpool.

R. O. WINSTEDT

**Land Tenure among the Garia: The Traditional System of a New Guinea People.** By Peter Lawrence. *Social Sci. Monogr.* No. 4. Canberra (Australian Nat. Univ.), 1955. Pp. 63. Price 7s. 9d.

The Garia are a population numbering about 2,500 now domiciled in 14 village communities in a mountainous region of New Guinea known as the Bagasin Area, south-west of Madang. This short monograph is an attempt to demonstrate the relationship between social structure and land rights among these people. The presentation is clear and crisply presented. Although parts of the analysis appear to be vulnerable to criticism, the fieldwork documentation is sufficiently detailed to permit constructive reanalysis.

According to Dr. Lawrence, primary rights in land are vested in patrilineages, usually about four generations in depth. The individual male can, however, often exercise claims in land areas which belong primarily to patrilineages linked to his own by affinal ties—e.g. the patrilineages of his mother, his father's mother, and his father's father's mother. Analysed in these terms the resulting pattern of land use and residence is highly complicated—so complicated indeed that the present writer feels considerable doubts as to whether Dr. Lawrence has really fully understood the nature of the system he describes.

One or two particular difficulties may be mentioned. Rights in land are inherited by the individual from his parents, but we are not told how these rights first come to be established. The land-right-owning corporation is a 'named patrilineage'; these patrilineages seldom exceed four generations in depth; we are not told how or why lineages then segment, nor how new named patrilineages are formed, nor what is the principle of segmentation. In a diagram facing p. 28, we are told that land becomes finally vested in an affinal (sister's son's) patrilineage five generations after the original transfer. This is said to occur four generations after the Garia have forgotten the name of the ancestor who first made the transfer. I find it extremely difficult to believe that Garia really argue in this way about the activities of ancestors whose names they do not know. These points are sufficient to demonstrate the difficulty of accepting Dr. Lawrence's analysis as it stands. I myself get the impression that he has perhaps been too eager to detect a straightforward system of patrilineal descent in this society. An analysis which distinguished more precisely the kinds of right that are transmitted through men and the kinds of right that are transmitted through women might perhaps be more successful. The pattern of residential grouping has been seriously disturbed in recent years by arbitrary Administrative action but it is evident that individual Garia have wide choice as to where they establish their permanent domicile. The impression I get is that the continuing units in Garia society are the residential hamlets and their associated lands rather than patrilineal corporations defined by descent.

This is a stimulating monograph. Its limitations are of a kind which challenge further enquiry. The features of social structure which Dr. Lawrence endeavours to analyse are by no means confined to New Guinea.

E. R. LEACH

## CORRESPONDENCE

**Stone and Clay Missiles in Buganda.** Cf. MAN, 1955, 81

**33** Sir.—Mr. Lanning's note about the use of spherical stones and clay balls for defence is very interesting. I have come across similar clay balls twice in my experience in Uganda, once on one of the islands in Lake Kyoga and again at a

depth of some 3 feet in the soil in Soroti township, an old inhabited site in Teso District, when digging a pit latrine. (These balls were collected and for all I know may be still at the bottom of the 'games locker' in the correspondence clerk's office in the Soroti District Office.)

As regards the spherical stones, Mr. Lanning's description of their use as missiles does not explain the reason for their presence in the area. Neither granite nor amphibolite of itself cracks into spherical blobs. Their spherical shape is, therefore, inexplicable in the absence of (a) fast running streams (*cf.* David's collection of slingstones from the river-bed). There are no such streams in Western Uganda, except for those running off the Ruwenzori massif, which is miles away from the area where Mr. Lanning has been making his archaeological discoveries. The majority of Uganda streams are sluggish, treedy, or papyrus-grown, incapable of wearing stones at all; (b) deliberate human manufacture. It is difficult to believe manufacture like this would occur *ab initio* in view of the long and toilsome process required to shape granite stones, unless there was some definite secondary economic inducement.

In fact, however, a study of tribal movement provides a simple answer to the problem presented. The large Bantu community of the Sukuma-Nyamwezi group (who now live to the south of Lake Victoria), by tradition derives largely from clans who have migrated from Bunyoro, Toro, Ankole, Karagwe, Kiziba and Ruanda-Urundi. They are all grain-eaters and have an economy in which grinding is a factor of great importance. Convenient slabs of granite on the hillsides are littered with saucer-like depressions, where the women in the past (when the villages were all up on the hills for defensive purposes) used to congregate for grinding parties. I seem to recall records of similar depressions being found on various old habitation sites in the Western Province of Uganda and they are common further north in the Acholi and Lango country.

The grinding apparatus used among the Sukuma-Nyamwezi people today consists of three pieces of rock. First the large (female) saucer-like grindstone (*iwe*), which is derived from any large natural wedge of rock weighing about 30–60 pounds, which has cracked off the granite outcrops at the top of the hills. This is slung on a pole and carried down by two or more men to the plains, where the Sukuma today have their individual homesteads. The (male) grinder stone (*sho*) is usually a flattish ovoid weighing about 2–5 pounds. Lastly there is the 'sharpener stone' (*ng'homango* from *kukomanga*—'to sharpen'). This usually starts as any handy hard tough rock weighing about 2–3 pounds, which can be banged on to the grindstone to chip it into the desired saucer shape and later to 'touch it up' to give the surface the required roughness for quick grinding. The *ng'homango* usually finishes after long use as a neat sphere (varying in size from a cricket ball to a bowl) because every projection on it has been knocked off in the process of sharpening the grindstone. I have never seen in any other part of Africa as many of these stones as there are round the Sukuma homesteads where one can nearly always find two or three lying about in odd corners. Tradition relates that these stones, as well as smaller ones for slings (*itago*), were formerly used in village defence.

My suggestion is that at one time all the people living to the west of Lake Victoria were grain-eating, as are those today who live to the north of Lake Kyoga and to the south of Lake Victoria. Banana cultivation was an intrusion which by tradition spread outwards from Buziba and from old Buganda (*i.e.* the little kingdom that extended hardly more than fifty miles north and west from Kampala, and was bounded by the Nile and Lake Victoria). Mr. G. A. Wainwright in an article in MAN some years ago suggested that there were grounds for thinking that the banana had been brought from Ethiopia to Uganda. I do not believe this can have been so, for the following reasons: (a) the weight, bulk, and comparative fragility of the banana suckers, which have to be carried from place to place when bananas are propagated. (This would preclude foot portage across 'the bad lands' between Ethiopia and North Uganda); (b) the difference between the Ethiopian and Uganda varieties of banana; (c) the complete absence of traditional association of the Bachwezi or Lwoo invaders with the banana plant, which assuredly would have been a major detail of their cultural tradition if they really had introduced it; (d) the association of the banana with the legend of Kintu and the story of how the banana was brought by Nambi 'the daughter of the "King of Heaven"' from the Sesse Islands into Buganda.

All this suggests support for the late Mr. Hornell's theory (also published in MAN during the 1930's) that there was a migration

of people carrying some traces of Indonesian culture into this region. These traces included the banana, the beer-pipe, the beer-canoe, bark-cloth, the 'stepped' drum, the xylophone, the flat-back zither and the double-prowed canoe, also the Chinese coolie hat and a belief in the fertility value of pangolins. Their suggested route lay up the Zambezi River, the Shiré river, Lake Nyasa, Lake Tanganyika to Lake Victoria, where a secondary centre of diffusion was established on various defensible Lake islands. (This practice of settlement on islands is common among sea-going people. Compare the settlement of the East African coast islands by maritime settlers long before they attempted settling on the mainland.) The banana is an easy and natural plant to carry by canoe travelling from one lacustrine or riverain settlement to the next.

The culture of the Lake Victoria islands was almost entirely wiped out by sleeping sickness at the turn of the last century; but the banana culture had long before this spread eastwards and westwards wherever the rainfall allowed it, so that today banana cultivation extends in a solid block from Mt. Ruwenzori to Mt. Elgon, as far north as the Somerset Nile and the shores of Lake Kyoga and as far south as Buzinza at the south-west corner of Lake Victoria and Bukerewe at the south-east corner. Beyond this area both to the north and to the south, it is possible to cultivate the banana in favoured spots of the savannah zone; but it is eaten as a luxury, not as a staple food. Within this area, however, the banana has largely driven out grain cultivation and reduced it merely to a secondary crop for luxury beer-making. Consequently the people have largely forgotten the processes on which their ancestors' or predecessors' livelihood formerly depended, and Mr. Lanning's informants could only describe collecting the stones and not who made them.

I suggest that this explanation of how round stones came to be lying about the countryside in Western Uganda fits all the facts described by Mr. Lanning and provides also a confirmation of old tribal tradition. The only problem still to be settled is what was the grain these people grew? In Northern Uganda eleusine corocana (*wimbi*) is the old staple cereal, which is still grown in small quantities in the Western Province of Uganda. Among the Sukuma bulrush millet (*bubele*) is probably the old staple; but the introduction of sorghum (*busiga*) certainly took place at a fairly remote date and this has since become the staple food. In both areas maize is recent. Sorghum requires much more grinding than either bulrush millet or eleusine, which are both much smaller grains. This may account for the extreme commonness of the stone sharpeners today throughout the country to the south of Lake Victoria.

Mwanza, Lake Province, Tanganyika

A. C. A. WRIGHT

#### Choosing Reviewers. Cf. MAN, 1955, 102, 207

SIR.—I am moved, as an ethnographer, to run a tilt, very belated I fear, against Dr. Leach's thoughtless letter about choosing reviewers (MAN, 1955, 102). I use the term 'thoughtless' advisedly, as it is the kindest one in my vocabulary which will fit a letter that reads as though it were intended to be offensive, and I do not wish to incur editorial stricture.

That 'professional academic discipline' which Dr. Leach boasts has after all only come into existence as a development of the work of earlier men who had no such academic training—mere ethnographers such as Tylor or Haddon. Indeed, it is liable to suffer (if it does not already do so) by the very fact that it is academic, and must largely rely for its experience in the field on short visits instead of long residence like that enjoyed by such 'recorders of quaint manners and customs' as William Ellis, William Mariner, Alexander von Humboldt, George Catlin, or Charles Hose, to name the first five that occur to me of a great multitude of ethnographers not trained as professional social anthropologists.

I do not suppose Dr. Leach has ever encountered Henry Balfour's Frazer Lecture of 1937, so I recommend it to his attention, and I beg to remind him that the ethnographer is quite capable of being just as scientific a student of man as the social anthropologist produced by a particular academic discipline, which, after all, is far from being the only training ground of accurate observation and logical inference.

New Radnor

J. H. HUTTON





STONE SCULPTURE FROM SOUTHERN BRITISH COLUMBIA

# STONE SCULPTURE FROM SOUTHERN BRITISH COLUMBIA\*

by

MARIAN W. SMITH, M.A., PH.D.

*London School of Economics and Political Science*

**35** The five objects represented on Plate C were collected in Canada in the early part of the century but have not previously been noted in print. They belong to the Sir Alfred Bossom Collection and are now in the National Museum, Ottawa. They were not on view at the exhibition of the collection at the Imperial Institute in 1954.

Little is known of the cultural associations of the stone sculpture of the Pacific Northwest although an analysis of the art style has recently been made.<sup>1</sup> All the objects portrayed here come from the general Fraser River region, but specimens of somewhat similar character are also known from as far south as the Columbia River. The total area covered, however, is not great considering the exceptionally high quality of the art.

Wingert has distinguished two main styles: the Fraser River—Puget Sound sculpture and the Columbia River Valley sculpture, with no less than six subsidiary styles. The pieces shown here were not available to him at the time of his analysis. The bowl-like specimens (Plate Ca and b) are typical of Fraser sculpture; and mauls and/or pestles (Plate Cd) occur in both Fraser and Columbia sculpture. The bird head represented so simply and with such sophistication in the piece on Plate Cr seems to be more like the Yakima-Vantage style of Columbia sculpture, and the fine head with two interlocking faces shown in two views (Plate Ce and f) is reminiscent in concept at least of the maul head with three interlocking heads<sup>2</sup> which also belongs to the Yakima-Vantage region. Since the maul head at least can definitely be attributed to the salt-water reaches of the Fraser area, these two specimens suggest an interesting tie between the lower Fraser and the Yakima-Vantage region.

The age of the specimens is not known. It is generally felt, however, that the Fraser examples are older than those from the Columbia, some of which are, indeed, probably to be dated in the protohistoric period.

The meaning of the sculpture is, of course, a matter for speculation. Yet all indications point to the fact that the figures were more relevant to what may be called social ceremonial than to religion. There is no reason to believe that they are idols or represent gods. A hole, apparently for the suspension of the piece, is just visible in Plate Ch, and is plainly to be seen in Plate Cf. Similar holes occur in other Fraser bowl-like specimens.<sup>3</sup> It is not clear why bowls

should be suspended and there is no indication in the objects themselves of the purpose for which they were intended. It is tempting to think of them as related to the crest figures which are well known in the historic period among tribal groups somewhat farther north on the Northwest Coast.

*Plate Ca.* Identified as 'Salish,' probably from near Lytton. Height, 7 inches. Human figure grasping bird head, the rim of the bowl and edges of the human fingers and feet form the back of the bird head in which eye and beak are clearly portrayed. This is an example of consummate skill in dealing with interlocking figures. The rounded mouth persists in wooden masks and other figures up into the historical period.

*Plate Cb.* From 'Patricia Bay.' Height, 8½ inches. Probably a bird: the beak has been broken off, but wings are shown in relief on the sides. This is much simpler in conception than the other bowl-like specimen.

*Plate Cc.* Found at the mouth of the Cowichan River. Height, 2 inches. A highly stylized bird head; probably a pipe bowl. The quality of the sculpture comes out well in this piece which is small yet appears almost monumental in the photograph.

*Plate Cd.* No provenance given. Although badly worn, the head of a mountain sheep is clearly indicated with the curving horn, raised portion (to the left) from which the horns emerge, and eye. This piece is reminiscent of the mountain-sheep-head maul from the Deschutes—John Day area (Wingert, 1952, Plate 30), but the art style is not very similar.

*Plate Ce and f.* From 'South Pender Island, off Saanich Peninsula.' Height, 3½ inches. This is obviously the head of a pestle or club considerably smaller than the head in circumference: the spot at which the break occurred is visible in e and forms the base on which the head stands. This is a beautiful specimen in which a single eye serves for one side of each face. Looked at from either full face there is no suggestion of the head at the opposite side.

## Notes

\* Paul S. Wingert, *Prehistoric Stone Sculpture of the Pacific Northwest* (Portland Art Museum, 1952). See also this publication for bibliography.

<sup>1</sup> *Ibid.*, Plates 34, 35, 37.

<sup>2</sup> *Ibid.*, Plates 38, 40.

# THE DISTRIBUTION OF HÆMOGLOBIN C IN WEST AFRICA\*

by

DR. G. M. EDINGTON

*Medical Research Institute, Accra*

and

DR. H. LEHMANN

*St. Bartholomew's Hospital, London*

**36** Whereas it has long been known that the sickling phenomenon is often found in Africans or people of African origin, it has only recently been shown by Pauling and his colleagues<sup>1</sup> that sickle cells contain an abnormal haemoglobin: sickle-cell haemoglobin—haemoglobin S. Pauling used electrophoresis to separate S haemoglobin from normal adult haemoglobin (haemoglobin A). By this technique other variants of haemoglobin A have been discovered. Their mode of inheritance follows simple Mendelian rules, A and its variants being controlled by a series of allelomorphic genes. Haemoglobins D, G, H, I and J have so far been seen in a few families or even

AS, AC, AE. As haemoglobins S and C occur in the same populations heterozygotes for S and C can also be found.

A considerable number of surveys have been concerned with the distribution of haemoglobin S in Africa and indeed also in other parts of the world. Fig. 1 summarizes our present knowledge as far as the old world is concerned. As African slaves have been imported into the new world sickling is also a frequent feature in the West Indies and in North, Central and South America. An exhaustive table summarizing all surveys up to 1954 is published in A. E. Mourant's *The Distribution of the Human Blood Groups*,<sup>2</sup> and a general review of sickle-cell distribution with a discussion of the possible origin of the gene in Asia has appeared in the *Eugenics Review*.<sup>3</sup>

### *Hæmoglobin C*

Hæmoglobin C was discovered in a North American Negro,<sup>4, 5</sup> and in Africa was seen first in the Gold Coast.<sup>6</sup> This was not unexpected in view of the West African origin of the North American Negroes. Whereas haemoglobin S is widely distributed over the African continent north of the Zambezi river and south of the Sahara, haemoglobin C has not been found at any substantial frequency anywhere but in West Africa. Single instances have been seen in the Congo<sup>7</sup> and in South Africa,<sup>8</sup> and a number of families with hæmoglobin C have been found among Algerian Mussulmen.<sup>9</sup> These observations can readily be related to the importation of West African slaves to these regions. A survey of 200 Accra outpatients showed hæmoglobin C in 12 per cent.<sup>10</sup> (A similar survey in Boston showed C to be present in 2 per cent. of Negroes<sup>11</sup>). We have seen in Accra hæmoglobin C in a number of people from West African communities not included in the surveys recorded below: Ada, Akropong, Ashanti, Busanga, Grunshie, Frafra, Ibo, Kanjarga, Kardo, Nzimah, Zabrama. Of these the Busanga and Zabrama are derived from French territories north of the Gold Coast, and the Ibo from Eastern Nigeria. We have also seen hæmoglobin C in people originating from Sierra Leone.

Hæmoglobin C was not found in the Southern Sudan,<sup>12</sup> Uganda<sup>13</sup> or in Tanganyika.<sup>14</sup> The only region besides the Gold Coast where hæmoglobin C was seen at some frequency was Nigeria where it is present in the Yoruba (7 per cent.) but not west of the river Niger in the Igala.<sup>15</sup> However, as mentioned above, C has been seen in Ibos from Eastern Nigeria residing in Accra.

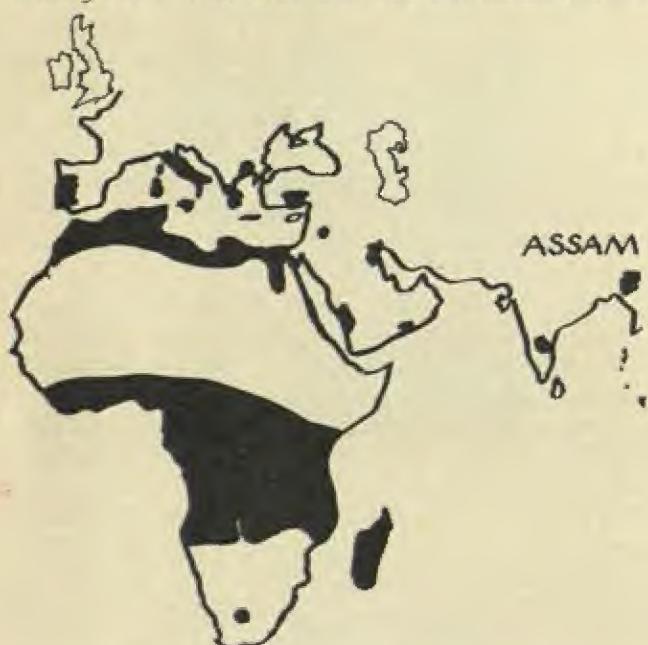


FIG. 1. DISTRIBUTION OF SICKLE CELLS

Sickle cells are found in Africa mostly south of the Sahara and north of the River Zambezi. With the exception of Spain sickle cells have been seen in all the major Mediterranean countries, in the Middle East and in South India and Assam.

individuals only, but C and E seem to be more widely distributed and are therefore of anthropological interest. E has been found at high frequency in South-East Asia, and C in West Africa. With rare exceptions electrophoretic analysis can show whether people are homozygous for the genes responsible for A or S, C or E, or heterozygotes for

\* With three text figures and four tables

We wish to record further data on the distribution of haemoglobin C in the Gold Coast, where we found a considerable difference between the inhabitants of the Southern and Northern Territories. All subjects were unrelated and were not suffering from obvious disease. Table I shows our findings in 183 of the first and 283 of the second.

TABLE I. HÆMOGLOBIN VARIANTS IN 466 AFRICANS FROM THE GOLD COAST

Area	Total	AA	AS	AC	SC	CC	AG		
	Number	No.	%	No.	%	No.	%	No.	%
Southern	183	131	71.6	34	18.6	16	8.7	1	0.5
Northern	283	205	72.4	37	6.0	57	20.1	2	0.7

It will be seen that the total frequency of people with abnormal haemoglobin was similar in both groups, but that the distribution of the S and C haemoglobins was very different.

TABLE II. PROPORTION OF PEOPLE WITH ABNORMAL HÆMOGLOBINS

Area	Total	Per Cent.		
		Abn. Hem.	S	C
Southern	183	28.4	19.1	9.8
Northern	283	27.6	6.7	21.5

The composition of the southern group was as follows:

TABLE III. COMPOSITION OF THE SOUTHERN GROUP

Community	Total	Per Cent.				
		AA	AS	AC	SC	AG
Ewe	46	30	10	6	—	—
Fante	29	16	9	4	—	—
Dagomba	108	85	15	6	1	1
	(73.7%)	(13.9%)	(5.6%)	(0.9%)	(0.9%)	(0.9%)

The samples from Ewe and Fante are small, but it will be seen that in all three southern communities S was more frequent than C. In the northern group, on the other hand, all three communities showed a higher incidence of C than S, a situation not known to occur in any other population. On comparing the northern communities with one another it was found once again that the total incidence of abnormal haemoglobins was similar in all three, though that of C was almost twice as high in the Dagomba as in the Dagarti.

TABLE IV. COMPOSITION OF THE NORTHERN GROUP

Community	Total	Per Cent.									
		AA	AS	AC	SC	CC					
Dagomba	71	48	67.6	3	4.2	19	26.8	—	—	1	1.4
Mossie	115	85	73.9	5	4.3	24	20.9	—	—	1	0.9
Dagarti	97	72	74.2	9	9.3	14	14.4	2	2.1	—	—

The C gene frequency in the Dagomba amounts to 14.8 per cent.; this compares with the higher ranges of S gene frequency seen anywhere in Africa or South India.

#### DISCUSSION

*Hæmoglobin S.* It will be seen that no S homozygotes were discovered. This survey was made on healthy unrelated individuals. Although we have of course seen numerous patients with sickle-cell anaemia, particularly

from the southern communities, none were found in this survey. We had at one time thought that we had discovered two S homozygotes among the Ga, who were healthy and who did not suffer from anaemia or other forms of sickle-cell disease.<sup>16</sup> Electrophoresis showed only S haemoglobin and some foetal haemoglobin (haemoglobin F). (The genetical control of F production is independent of that of haemoglobin A and its variants; whereas it normally disappears after the age of 4 months, it may persist beyond this age in certain haemoglobinopathies.) Although haemoglobin A could not be detected in these two Ga, family studies showed that the gene for its production was present.<sup>17</sup> It was possible to demonstrate the presence of a thalassæmia or thalassæmia-like gene which suppressed the formation of A haemoglobin. Thus although the blood was phenotypically indistinguishable from SS, the genotype was in fact AS.

*Hæmoglobin C.* As the gene responsible for haemoglobin S may give rise to disease and early death in homozygotes, an explanation had to be found for its presence at high frequency in many parts of tropical Africa. No evidence for an appropriately increased mutation rate for the S gene in these regions could be found.<sup>18</sup> Following Haldane's suggestion for the analogous case of thalassæmia<sup>19</sup> Beet,<sup>20</sup> Raper<sup>21</sup> and Brain<sup>22</sup> have proposed that the explanation may be a balanced polymorphism by which AS heterozygotes are more resistant to malaria than A homozygotes. By this means the excessive loss of the S gene by early death of S homozygotes would be balanced by a corresponding loss of the A gene in AA homozygotes dying at a greater rate than the AS heterozygotes from malaria. The whole subject is still controversial, the most positive evidence in favour of such a mechanism having been put forward by Allison.<sup>23</sup> Other workers<sup>24-26</sup> have not been able to confirm his findings fully, though most have done so in part.

Mourant<sup>2</sup> has suggested that the gene responsible for haemoglobin C may have arisen as a mutation for the S gene. Though C homozygotes may suffer from a mild haemolytic anaemia they are much fitter than S homozygotes, and they are well known to survive to adult age and to produce children. If, as is assumed for haemoglobin S, haemoglobin C shows a diminished ability to support malarial parasites highly specialized to utilize haemoglobin A, C would obviously be a much more viable variant genetically. It is possible that the mutation responsible for haemoglobin C has arisen somewhere in West Africa, and that we are witnessing its spread from north to south in the Gold Coast and from east to west in Nigeria. We have also seen haemoglobin C in people from Sierra Leone east of the Gold Coast, but a survey of incidence has not so far been carried out in that area. From the anthropological point of view, it would be expected that the results will at the most be similar to those obtained in the Southern Gold Coast rather than to those from the Northern Territories. Our present findings give the impression that haemoglobin C is replacing haemoglobin S in the Gold Coast.

For practical purposes any surveys relating sickling incidence with malarial infection should certainly include



FIG. 2. SICKLE BELT OF AFRICA

Sickling is distributed at high frequency with only few exceptions in the populations living south of the Sahara and north of the Zambezi River.

records of haemoglobin C in areas where this variant is present.

Figs. 2 and 3 summarize the difference in incidence of the S and C variants in Africa.

#### SUMMARY

Though the total proportion of abnormal haemoglobin carriers is similar in two groups from the northern and southern areas of the Gold Coast, a considerable difference is seen in the distribution of S and C haemoglobins, the latter being much more frequent in the northern group. The distribution of the C haemoglobin in Africa is discussed and it is suggested that it may be spreading from north to south in the Gold Coast and from there westwards into Nigeria.

#### Notes

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FIG. 3. THE DISTRIBUTION OF HÆMOGLOBIN C

The distribution of haemoglobin C is much more restricted than that of haemoglobin S. So far, C has been reported at a frequency comparable with that of S in tropical Africa only from the Gold Coast and Western Nigeria. Haemoglobin C has also been seen in some people from Algeria, Sierra Leone, French Equatorial Africa, the Belgian Congo and South Africa.

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## THE MAY TABU ON ROMAN MARRIAGE AND A PARALLEL

*by*

S. DAVIS

*Professor of Classics, University of Natal, Pietermaritzburg*

Marry in May  
And repent alway.

John Ray (English Proverbs).

Si te proverbia tangunt,  
mense malum Maio nubere volgus ait.  
Ovid, *Fasti*, V, 489-490.

**37** Plutarch<sup>1</sup> in his *Roman Questions* asks, 'Why did not the Romans marry in the month of May?' It is a curious fact that among the Romans on certain days of the year a wedding could not be celebrated. The first half of March, the whole of May, the first half of June, the Kalends, Nones and Ides of every month and the numerous Roman festivals were times when marriages were avoided. This avoidance was due to religious scruples which were prevalent in several parts of the Mediterranean area. In this article we shall be concerned only with the tabu that extended from May to the first half of June, and to showing that it was due to purificatory rites of a special kind arising from agricultural festivals.

In May, according to Plutarch, the Romans 'perform the greatest of purificatory ceremonies . . . they throw images from the bridge into the river [the Tiber], but in old times they used to throw human beings'.<sup>2</sup> These images they called *Argei*.<sup>3</sup> Ovid states that they were made of rushes.<sup>4</sup> For an explanation of this odd ceremony we have to turn to Greece. There the festival of the Thargelia at Athens, which also took place in May, throws light on the Roman rite, but we shall later find another parallel to it as well.

Although in Northern Europe the harvest is associated with the autumn, in Southern Europe it fell much earlier. Thus in Greece the grain harvest fell in the month Thargelion, i.e. in the latter half of May and the beginning of June, and was marked by three festivals, the Thargelia, Kallynteria and Plynteria. From Athenaeus we learn that 'the *Thargelos*' is the first loaf made after the carrying home of the harvest.<sup>5</sup> The meaning of the word Thargelia is thus clear and the festival is chiefly an offering of first fruits on the occasion of the harvest. But there is another element in this festival—the leading out of the *pharmakos*. 'At Athens they led out two men to be purifications for the city . . . at the Thargelia; one was for the men, and the other for the women'.<sup>6</sup> The *pharmakos* is thus a human scapegoat and the ceremony of purification was an apotropaic one to avert diseases.<sup>7</sup> The *pharmakos* was certainly scourged, but whether he was put to death at Athens in the fifth century B.C. is uncertain, though he was in primitive times. Jane Harrison sums up the whole idea of the *pharmakos* ceremony rather well<sup>8</sup>: 'The notion, so foreign

to our scientific habit of thought, so familiar to the ancients, was that evil of all kinds was a physical infection that could be caught and transferred; it was highly catching. Next some logical savage saw that the notion could be utilized for artificial riddance. . . . As the gist of the ceremony is magical riddance, it is essential that the scapegoat, whatever form he takes, should never return'.<sup>9</sup>

The full significance, however, of the *pharmakos* rite can only be realized if one recognizes that, since the Thargelia was an early harvest festival celebrated in May, the *pharmakos* was a representative of the creative and fertilizing god of vegetation,<sup>10</sup> annually slain to maintain the divine life in perpetual vigour untainted by age. Frazer explains the choice of a dying vegetation god as scapegoat as due to the combination of two originally independent customs. On the one hand there was a widespread custom, dating from a remote antiquity, of killing the human or animal vegetation god in order to save his divine life from being weakened by age; on the other hand it was customary to have a general expulsion of evils and sins once a year. The combination of the two customs resulted in the employment of the dying god as a scapegoat. Eventually 'the divine character of the animal or man is forgotten and he comes to be regarded merely as an ordinary victim. This is especially likely to be the case when it is a divine man who is killed. For when a nation becomes civilized, if it does not drop human sacrifice altogether, it at least selects as victims only such wretches as would be put to death at any rate. Thus the killing of a god may sometimes come to be confounded with the killing of a criminal'.<sup>11</sup> Most probably 'dying god' and 'scapegoat' were originally one and the same thing and need not be explained—as Frazer does—as arising from two originally distinct customs. In course of time the two aspects came to be separately emphasized, one to the neglect of the other, 'dying god' in one region of the ancient world and '*pharmakos*' in another.

At Athens the festivals of Kallynteria and Plynteria are closely connected with the Thargelia. The Kallynteria was virtually a festival of 'early summer cleaning' when the sacred places were swept out and cleaned and the Plynteria took place when the sacred image of Pallas was washed. 'These two festivals throw light on the purport of the *pharmakos*, and emphasize the fact that all the cleansing, whether of image, sanctuary or people, was but a preliminary to the bringing in of the first fruits'<sup>12</sup> in the early summer.

To return to the Roman practice in the month of May. The *Argei* at Rome were clearly *pharmakoi* and though, on Ovid's authority, they were made of rushes,<sup>13</sup> they had

originally been in 'old times' human beings,<sup>14</sup> as was the case at Athens and elsewhere.

There is a close resemblance between the throwing of these straw images into the Tiber and the numerous vegetation rites in which a puppet of some kind is carried about in procession, ducked or sometimes put through a form of execution such as beheading. These practices most probably represent the carrying-out of the dead or dying vegetation spirit and it is noticeable that the puppets are usually represented as old men or women.<sup>15</sup> In this connexion it is interesting to notice that the proverb '*sexagenarios de ponte*',<sup>16</sup> i.e. 'throw the men of sixty off the bridge,' was taken to refer to the *Argei* who were thrown into the Tiber on 15 May, i.e. in the summer. Among the Hebrews and the Egyptians<sup>17</sup> surrogates (i.e. substitutes for human beings), as in the case of the *Argei*, were also made use of, a goat<sup>18</sup> in the one case and an ox in the other.<sup>19</sup>

It is pertinent to our case to state that at Rome another *pharmakos* or scapegoat ceremony, that of Marmurius Veturius, who represented Mars as the *numen* of nature or the year spirit, took place even earlier, in the spring, about a fortnight after the opening of the old Roman year. Every year on 14 March a man clad in skins was led in procession through the streets of Rome, beaten with long white rods and driven out of the city.<sup>20</sup> He was called Marmurius Veturius, which may mean 'the old Mars.' As the ceremony took place on the day before the first full moon of the old Roman year (which began on 1 March,<sup>21</sup> the skin-clad man probably represented the Mars of the old year, who was driven out at the beginning of a new one. Mars was originally not a god of war but of vegetation,<sup>22</sup> as is proved by the following facts. The Arval Brothers, whose duty it was to make sacrifices for the growth of the crops, addressed their petitions to him. Moreover, the dedication of the spring month of March to Mars is an indication that he was the deity of sprouting vegetation. It is significant that Marmurius Veturius, the representative of the god, appears to have been treated not only as a deity of vegetation but also as a scapegoat, for his expulsion from Rome implies this.<sup>23</sup> At Rome the festival of the Vestalia, though falling a little later than the Kallynteria and Plynteria at Athens, has the same content as those festivals. On 7 June the innermost sanctuary of Vesta, closed all the year round, was opened to matrons for seven days and the Vestals offered sacred cakes made of the first ears of corn, plucked in the early days of May. On 15 June the temple of Vesta was swept and the refuse taken away or thrown into the Tiber.

The origin of the cult of Vesta is of great interest—it lay in the necessity in the primitive community of keeping one fire always alight, so that its members could at any time supply themselves with it as it was hard to obtain it at a moment's notice. The task of keeping the fire alight was that of the chief's daughters. At Rome 'the Vestals were under the *patria potestas* of the *pontifex maximus*, who represented in Republican times the legal powers of the *Rex*, and from this fact we may safely argue that they had once been the daughters of the primitive chief.'<sup>24</sup>

Although the germ of the cult of Vesta was the perpetual

fire in the *aedes Vestae*, adjoining the king's house (*regia*), it was constantly connected with the fruits of the earth. Among certain peoples living by agriculture even today, as was originally the case with the highly civilized societies of Greece and Rome, special importance is attached to the days immediately before harvest and the gathering of the first fruits 'at which time there is a general cleaning out of house, barns, and all receptacles and utensils, and following upon this a period of rejoicing.'<sup>25</sup>

Frazer<sup>26</sup> gives an illuminating parallel to the cult of Vesta from among the Creek Indians of North America. The chief points in his account are: (1) the solemn nature of the whole rite as shown by a general fast; (2) the idea that purification is necessary before the first fruits are brought in—a purification of human beings, houses and utensils; (3) the renewal of the sacred fire which coincides with the beginning of a new year; (4) the bringing in of the first fruits in a solemn ceremony. The first two are represented at the Vestalia by the religious nature of the days and the mourning of the Flaminica Dialis, and by the cleansing of the *penus Vestae* (store house). Although at Rome the third is not represented at the Vestalia save on 1 March, the beginning of the Roman year, and the fourth only by the plucking of the first ears of corn by the Vestals before the Ides of May, in order to make the sacred salt cakes of sacrifice, yet it is possible, as Warde Fowler remarks, 'that the Roman ceremonies of March, May and June were three parts of one and the same rite which in course of time had been separated and attached to different periods of the year.'<sup>27</sup>

At Rome the whole period from 1 May till the middle of June was regarded as unlucky for marriages for the very reason that the period was one of purification. Plutarch<sup>28</sup> had already hit upon the right answer as regards May when he said: 'May it be that in this month they perform the greatest of purificatory ceremonies?' This was, as we have seen, the ceremony of the *Argei*. Ovid shows us that not until the festival of the Vestalia was completed was the tabu on marriage in this period lifted. When he consulted the Flaminica Dialis as to the marriage of his daughter, he was told that till the Ides (13th) of June the period was unlucky for marriages: 'Until the calm Tiber shall have carried down to the sea on its yellow current the filth from the temple of Ilian Vesta, it is not lawful for me to comb down my hair with a toothed comb, or cut my nails with iron, or touch my husband, though he is the priest of Jupiter, and though he was given to me for life. Thou too be in no hurry; thy daughter will better wed when Vesta's fire shall shine on a clean floor.'<sup>29</sup>

There is a parallel from across the Mediterranean to a tabu of this nature against marriages in May and the first half of June.

Among the ancient Hebrews there probably was an interdict against marriage for the period roughly between the middle of April and the first week of June. Among the Jews of today marriage is forbidden in this period, i.e. in the interval between Passover and the Feast of Weeks (Shevuoth, Pentecost) except for one day, the thirty-third day after Passover. The reason usually given is that it is a

season replete with sad memories since massacres of Jews took place in this period of the year during the reign of the emperor Hadrian and during the Crusades. The real reason, however, has, in modern times, been obscured and forgotten. The ban on marriages during the period was probably due to the same reason as at Rome—purificatory rites attendant upon the spring and early summer festivals. In Palestine first fruits were offered, chiefly of the barley at the former season, and of the wheat at the later season.

In Palestine the Passover festival, which began on the night of the full moon of the month nearest the Spring Equinox, the fourteenth of Nisan (or Abib=Spring),<sup>30</sup> i.e. about the middle of April, was a complex spring festival which contained three distinct rites. The first was the Passover proper, the Paschal meal, which according to one theory was originally an ancient nomad household festival.<sup>31</sup> A sheep or a goat, a firstling, was killed and eaten entirely at a sacrificial meal at night and its blood smeared on the outside of the dwelling place of the worshippers. The explanation of this rite which connects it with the Exodus is demonstrably late.<sup>32</sup> It was a world-wide custom among primitive peoples to offer the firstlings of the flocks and herds, later the first fruits, to the deity, to ensure their increase during the coming year. 'The object of the sacrificial meal at which the victim was eaten was to absorb the divine life conceived to be present in the sacred victim and thus to become united with the deity.'<sup>33</sup>

The second rite—and more important for our purpose—was the purely agricultural festival of the unleavened bread lasting seven days which fell at the same time of the year as, and was ultimately combined with, the Passover.<sup>34</sup> The explanation that the Israelites at the Exodus had in their hurry no time to bake leavened bread is not the true one.<sup>35</sup> The real explanation is that the Exodus took place at the time of this festival and so was later connected with it.

'Leavened bread,' says Jane Harrison, 'is a product of advanced civilization and with a true conservative ritual instinct the Roman church prescribes to this day the use of the unleavened wafer.'<sup>36</sup> The Athenians ate barley porridge in remembrance of their ancient mode of life as did the Hebrews unleavened bread.<sup>37</sup> Solon in fact bids the Athenians supply to those who had free meals in the Prytaneum barley cake.<sup>38</sup> The Greek word for barley bread or cake, *pózca*, is astonishingly like the Hebrew word for unleavened bread (*massa*; modern pronunciation *matsa*). Thus we are not surprised to find that the third rite of the Passover was an offering of the first-cut barley sheaf (the *omer*) at this spring festival.<sup>39</sup>

In Greece in the early summer, the Thargelia, as already stated, was chiefly an offering of first fruits on the occasion of the wheat harvest, with which was associated the magical riddance ceremony of the *pharmakos* and the cleansing ceremonies of the Kallynteria and Plynteria. The *pharmakos* ceremony, as has been shown, throws light on that of the *Argei*, and the Kallynteria and Plynteria, which are associated with the Thargelia, on the Vestalia at Rome. They have all the same content. There is also a Palestinian parallel to the Greek early summer festival of the wheat

harvest. It is the Feast of Weeks which occurs seven full weeks after the beginning of the barley harvest, i.e. after the second day of Passover, and is identified with the fiftieth day of the counting of the Omer,<sup>40</sup> for between the offering of the first cut barley sheaf (*omer*) at Passover and the offering of the first wheat sheaf at the Feast of Weeks,<sup>41</sup> fifty days (hence Pentecost) are supposed to be counted. During this period of counting (*Sephirah*) the days between Passover and the Feast of Weeks, i.e. from about the middle of April to the first week in June, no marriages were allowed except on the thirty-third day of the counting.

The rite at the Feast of Weeks in the early summer may originally have consisted in the offering of a sheaf of wheat analogous to the sheaf-offering of barley at the commencement of the barley harvest in the spring. It was originally only a few weeks after the Passover.<sup>42</sup> At any rate at a later time the first fruits consisted of the first two loaves made out of the new wheat<sup>43</sup> analogous perhaps to the original intention of the Feast of Unleavened Bread. In the Priestly Code the sacrifices were the same as on the seven days of Massoth.<sup>44</sup>

Now, as we have seen, at the Thargelia in Greece we find cleansing ceremonies associated with the early summer festivals, viz. the Kallynteria and Plynteria, and at Rome, not long after the ceremony of the *Argei*, the cleansing ceremony of the Vestalia. Have we cleansing rites too at the Feast of Weeks? There is no evidence for such rites at this Festival, though there may well have been at one time. There is evidence, however, of cleansing rites at the Passover which may have antedated the Feast of Weeks by only a few weeks originally and not by seven.

The cleansing rites at the Passover are as follows. On the evening preceding the fourteenth day of the month Nisan, immediately after the evening service, the master of the house searches after and gathers all the leaven lying in his way. During the search strict silence is observed (*savete linguis!*). On the fourteenth day about 10 o'clock in the morning all leaven must be removed and that gathered the preceding evening must be burnt. The procedure is accompanied by appropriate prayers.

Furthermore, in every Jewish household there are two sets of crockery and cooking utensils—one for use all the year round and the other used only for the Passover week. However, instead of using a new set of utensils for Passover the old may be rendered fit for this purpose only by a special ritual of cleaning and immersing in boiling water to destroy the smallest particle of leaven adhering to them. On the actual eve of Passover, i.e. on 14 Nisan, a general fast is observed exemption from which, among the Ashkenazic Jews, is obtained by joining in the religious meal of the *Siyyum* (a joyous occasion to denote the completion of the study of a tractate of the Talmud).<sup>45</sup>

Not only were private houses cleansed but all buildings, even to the sepulchres lying outside Jerusalem, which were given an annual coat of whitening so that they shone in the sun 'and thus lessened the risk of ceremonial defilement'.<sup>46</sup>

Several of the characteristics of the Passover festival find a parallel in the 'busk' or festival of first fruits among the

Creek Indians of North America which, as has already been remarked on, bears a close resemblance to the Vestalia and other Roman rites. The Creek Indians provided themselves, before their festival took place, with new clothes and household utensils, burned all their old clothes, rubbish, grain and old provisions in a common heap, while the women scoured all the cooking vessels to receive the new fire and the new fruits; a general fast then followed.<sup>47</sup>

The whole procedure at the Passover is then nothing else than a ritual spring cleaning (the month Nisan as already indicated is the Babylonian name for the Biblical month Abib which means 'spring') and a return to the use of the ancestral food which was, as we have seen, not leavened but unleavened. In fact the festival of unleavened bread seems to have originated from an old religious custom that all bread offered to Jahweh was to be without leaven.<sup>48</sup> There is a general principle as to the material of sacrifice to the divinity—the god fares as his worshipper but sometimes he fares worse.<sup>49</sup> Men begin by offering just what they eat themselves. As they advance in civilization, 'their own food becomes more delicate and complex but they dare not make any change in the diet of their gods: they have learnt to bake and eat fermented bread themselves, but the gods are still nurtured on barley grains and porridge.'<sup>50</sup> Animal sacrifice comes in much later and in Homer never takes place without the previous sacrifice of grain-sprinkling and prayer.<sup>51</sup>

Cleansing rites were thus a part of the Passover festival. But have we also to expect at this spring festival, as was the case in Greece at the summer festival of the Thargelia, and at Rome with the ceremonies of Marmurius Veturius in the spring and of the Argei in the summer, a magical *pharmakos* ceremony to be explained as a 'dying vegetation god' ritual combined with the idea of a 'scapegoat,' who takes all the sins of the community for the past year on his own head? Frazer holds that such was the case. He sees in the Jewish festival of Purim a dying vegetation god in Haman who is represented complete with all the paraphernalia of the *pharmakos* and identified too with the substitute for the divine king who is the vegetation spirit annually put to death.

In one of the later stages of the development of this ritual the substitute came to be a condemned criminal, as, for instance, the *pharmakos* at Athens and elsewhere. Yet even a criminal eventually 'comes to be thought too good to personate a god on the gallows or in the fire; and then there is nothing left but to make up a more or less grotesque effigy, and so to hang, burn or otherwise destroy, the god, in the person of this sorry representative,'<sup>52</sup> as, for instance, effigies of Haman at Purim among the Jews and the Argei at Rome.

Frazer traces the Purim festival back to the festival of Zakkum (the Sactea, perhaps, of Berossus) celebrated at the Babylonian New Year. Although Purim is traditionally held in the middle of the last month of the Jewish Year, Adar (March), it corresponded nearly enough with the date of the Babylonian festival, to which it bears a striking resemblance. The latter fell a fortnight later than Purim, in the early days of the following month, Nisan, the first

month of the New Year, on the fourteenth of which fell the Hebrew Passover. Furthermore, 'A trace of the original celebration of Purim in Nisan may perhaps be found in the statement that "they cast Pur, that is the lot, before Haman in Nisan the first month of the year."<sup>53</sup> It has been suggested with some plausibility that the Jews may have shifted the date of Purim in order that the new and foreign festival<sup>54</sup> might not clash with their own old festival of the Passover which began on the fourteenth day of Nisan.'<sup>55</sup>

In any case according to the New Testament there was a Jewish custom, which is not, however, mentioned in any Hebrew source, whereby a prisoner was released at the Passover. Pilate at the trial of Jesus says to the Jews, 'Ye have a custom, that I should release unto you one at the Passover.'<sup>56</sup> The custom was originally Babylonian, and was borrowed by the Jews after the Babylonian exile.<sup>57</sup> The old Assyrian calendar states that the king had to release a prisoner on the sixteenth of Adar, the last month of the year, the month before Nisan. The release of a prisoner at this time was an old Babylonian regulation introduced into the calendar of Adar, the harvest month, before the tenth century B.C. There may be a direct connexion between the Hebrew Passover and the Babylonian calendar since 'the two Babylonian fast days, seven days apart, covering the period 13-20 in the harvest month (Adar) is suspiciously like the Hebrew regulation of the Feast of Unleavened Bread from Nisan 14-20.'<sup>58</sup>

Was the prisoner released at the Passover a *pharmakos*? Frazer is of the opinion that he was and in the appendix to his *The Scapegoat* makes out what appears to be a fairly strong case. 'It was customary, we may suppose,' Frazer states, 'with the Jews at Purim or perhaps occasionally at Passover, to employ two prisoners to act the parts respectively of Haman and Mordecai in the passion play which formed a central feature of the festival. Both men paraded for a short time in the insignia of royalty, but their fates were different; for while at the end of the performance the one who played Haman was hanged or crucified, the one who personated Mordecai and bore in popular parlance the title of Barabbas was allowed to go free.'<sup>59</sup> The two characters according to Frazer are simply two different aspects of the same deity considered at one time as dead and at another as risen, and this deity is the god of vegetation who represents the drama of the annual death and regeneration of the products of the earth—a conception that was prevalent all over the ancient Mediterranean world. We may confidently assume that there were two features in the *pharmakos* concept—an old vegetation god who dies, and a new vegetation god who is really the old who rises in his place. The second feature seems to have been forgotten or fell out of use when the idea of the old vegetation god as a scapegoat became the predominant idea. The *pharmakos*, it must be emphasized, is always, when found, inseparably connected with harvest festivals in the spring and in the summer, and purificatory rites and tabus of various kinds are characteristic of these festivals.

To recapitulate, not only at Rome but also in Palestine

there was a *pharmakos* ceremony in the spring; furthermore, both at Rome and at Athens there were *pharmakos* ceremonies in the summer as well. The *pharmakos* and other cleansing ceremonies which took place in the spring were, at Rome, that of Marmurius Veturius, in Palestine, Purim and Passover; and in the summer, at Athens, Thargelia, Kallynteria and Plynteria, at Rome, Argei and Vestalia, and in Palestine Shevuoth (Pentecost). Although no actual *pharmakos* ceremony, as far as we know, is associated with the last, it is not absolutely essential to our argument since if Shevuoth, as already indicated, originally followed Passover after a much shorter period than is now the case, the same *pharmakos* ceremony of Purim, which served for Passover, would have served for it as well.

We may conclude by saying that the aim of sacrifice at the spring and early summer festivals in Greece, Rome and Palestine was purification, in which the *pharmakos*, originally the dying vegetation god but now the scapegoat who takes the sins of the community upon his own head, played no mean part—a cleansing to prepare for the incoming of the first fruits, and that this had an important corollary: the ban on marriages within a defined period. Plutarch then is right in his assertion that the marriage tabu at Rome was due to a rite of this kind. As in Greece with the Kallynteria and Plynteria and at Rome with the ceremonies of the *Argei* and the Vestalia, so too in Palestine cleansing rites at the Feast of Unleavened Bread were purificatory rites, which in a sense lasted till after the Feast of Weeks in June, as the marriage tabu shows. Till these rites have been completed, the whole period, from the beginning to the end of the rites, is regarded as sacred and therefore unpropitious for marriage, i.e. a period roughly from mid April to early June in Palestine, and from 1 May to mid June at Rome. All this, it must be emphasized, seems to point in some ways to a remarkable homogeneity of culture throughout Southern Europe and Western Asia, in lands in which, as we know well, the seeds of our civilization were planted.

## Notes

- <sup>1</sup> Q.R., LXXXVI.
- <sup>2</sup> *Ibidem*.
- <sup>3</sup> *Ibidem*, XXXII.
- <sup>4</sup> Ovid, *Fasti*, V, 621.
- <sup>5</sup> Athenaeus III, 52, p. 115: Θάρυγλον Καλείσθαι τὸν ἐκ τῆς συγκομιδῆς πρῶτον γενόμενον δρότον.
- <sup>6</sup> Harpocrates, s.v.
- <sup>7</sup> Helladius, *ap. Phot.*, Bibl., c. 279, p. 534.
- <sup>8</sup> J. E. Harrison, *Prolegomena to the Study of Greek Religion*, 2nd edn., Cambridge, 1908, p. 103.
- <sup>9</sup> Cf. *Leviticus*, XVI, 21, and Herodotus, II, 39, for the Hebrew scapegoat and the Egyptian scape-animal.
- <sup>10</sup> Sir J. G. Frazer, *Golden Bough*, abridged edn., London, 1929, p. 580: 'The object of beating the human scapegoat on the genital organs with squills and so on must have been to release his reproductive energies.'
- <sup>11</sup> Frazer, *op. cit.*, p. 376.
- <sup>12</sup> J. Harrison, *op. cit.*, p. 116.
- <sup>13</sup> *Fasti*, V, 621.
- <sup>14</sup> Plutarch, Q.R., LXXXVI.
- <sup>15</sup> Plutarch, *Roman Questions*, edited by H. J. Rose, Oxford, 1924, p. 99.
- <sup>16</sup> Cf. Festus, s.v. *Argei*; Cicero, *Pro Rose.*, 35, 100.

<sup>17</sup> It may be noted that in the first-fruits ceremony Osiris had already taught the Egyptians to substitute corn for human beings.

<sup>18</sup> *Leviticus*, XVI, 21.

<sup>19</sup> Herodotus, II, 39.

<sup>20</sup> Frazer, *op. cit.*, pp. 577-579. Cf. Warde Fowler, *The Roman Festivals*, London, 1899, pp. 44ff.

<sup>21</sup> Cf. S. Langdon, *Babylonian Menologies and the Semitic Calendars* (The Schweich Lectures of the British Academy), London, 1933, p. 55. According to a fragment of a small ivory prism from Babylon, now in the British Museum, the Babylonians in the seventh century B.C. fixed the beginning of the year at the new moon near the spring equinox. The latter took place on 21 March. 'The great church calendars of Assyria and Babylonia... are based upon a year beginning with March, precisely as did the religious calendar of Rome.' This calendar was Sumerian in origin and Babylonia, Assyria and Judaea (after the Babylonian exile) passed it on to the West.

<sup>22</sup> Warde Fowler, *op. cit.*, pp. 33-35, 44-56; *Oxford Classical Dictionary*, s.v. Mars.

<sup>23</sup> Frazer, *op. cit.*, p. 578.

<sup>24</sup> Warde Fowler, *op. cit.*, p. 147. Cf. Langdon, *op. cit.*, p. 117: 'As to the Roman calendar, where June corresponds to Sivan, the principal ceremony was the Vestalia, June 9, the celebration of Vesta, goddess of household fires. The period June 5-14, until the temple of Vesta was cleansed on the 15th, was nefast in Rome, when no marriages could take place. The Vestalia was clearly a fire cult and since it fell in the same month as that sacred to the fire-god in Sumer and Babylonia there may well be a connexion here.'

<sup>25</sup> Warde Fowler, *op. cit.*, p. 151.

<sup>26</sup> Frazer, *Golden Bough*, III, *Taboo and the Perils of the Soul*, p. 75 (or abridged edn., p. 485).

<sup>27</sup> Warde Fowler, *op. cit.*, p. 153.

<sup>28</sup> Q.R., LXXXVII, 3.

<sup>29</sup> Ovid, *Fasti*, VI, 219-254.

<sup>30</sup> Cf. S. Langdon, *op. cit.*, pp. 21f. The Jews during their Babylonian exile took over the Babylonian names of the months. Abib is the Biblical (Canaanite) name for the spring month which was replaced by the Babylonian Nisan. *Ibidem*, p. 24: 'A stone slab inscribed with a list of Canaanite months was discovered at Gezer, but it is uncertain whether it is intended to be a real calendar or merely a promiscuous list of the agricultural activities of the year.' Although two months are missing the list is relevant to our purpose and is given here: (1) 'osep, ingathering, October; (2) zera', sowing, December; (3) lekel, late sowing, February; (4) 'asad pelet, harvesting of flax, March; (5) kusar se 'orim, harvesting of barley, April; (6) kusar kol, harvesting of all (the rest), May; (7) zamar, pruning vines, June; (8) kes, fruit harvest.

*Ibidem*, p. 97: 'The Sumerians divided the twelve lunar months into two parts, precisely as did the Egyptians. There is consequently a second New Year in the Sumerian and Babylonian calendars. The custom of beginning the year in the autumn after the feast of ingathering of the last fruits, the *hag ha' asip*, at the end of the year, was the original Hebrew custom. The Canaanites began the year with the feast of ingathering, 'osep. Not until the Babylonian Exile did the Hebrews adopt Nisan or the spring equinox as the first month. Abib, the old Hebrew name for Nisan, was the seventh month originally.'

<sup>31</sup> Oesterley and Robinson, *A History of Israel*, Vol. I, Oxford, 1948, p. 423.

<sup>32</sup> Oesterley and Robinson, *Hebrew Religion: Its Origin and Development*, London (S.P.C.K.), 1949, p. 130.

<sup>33</sup> Oesterley and Robinson, *Hebrew Religion*, p. 131: 'As the festival was celebrated at full moon, the deity to whom the sacrifice was made was the moon.'

<sup>34</sup> Cf. Langdon, *op. cit.*, p. 96: 'The great festival of Nisan in the Jewish calendar, the offering of first-fruits and first-born of the herds, was fixed by Ezekiel on the 14th of this month. Now the old Assyrian calendar marks the 14th of Nisan 11 by the "bringing-in" of offerings, and so does the reformed calendar of the seventh century B.C. It has been presumed that the Hebrew Passover and Feast of Unleavened Bread had been finally fixed on the 14th of

Nisan because it falls on the day of the full moon. Ezekiel may have had this in mind, but the Assyrian note on the offering of first-fruits of the harvest on this day is as old as the tenth century B.C. and probably much more ancient. Ezekiel almost certainly obtained the date of the Passover direct from the Babylonian Church year-books.<sup>15</sup>

<sup>15</sup> *Deuteronomy*, XVI, 1-8. Cf. H. E. Jacob, *Six Thousand Years of Bread*, New York (Doubleday, Doran), 1941, pp. 37f. The seven-day period of eating unleavened bread (*Matzoth*) at the Passover was not a memorial of the Flight from Egypt (*Exodus*). This was an etiological explanation of the custom. Unleavened bread, not leavened, was in fact the earliest form of bread eaten and the custom of eating it was older than Moses. When Lot, who lived several hundred years before the Exodus, was visited by the angels, he offered them unleavened bread (*ugoth matzoth*), although he dwelt in a settled house and had time to finish baking his bread. It seems that at any time during the year the profane yeast bread could be eaten, but in the presence of God or his emissaries, only sacred bread could be eaten—cakes that had not been made with sour dough. Jahweh permitted only unleavened bread to be placed upon his altar. In Rome, also, the Flamen Dialis, the high priest of Jupiter, was forbidden to touch *farinam fermento imbutam* (flour mingled with leavening).

<sup>16</sup> J. Harrison, *op. cit.*, p. 88.

<sup>17</sup> Cf. H. E. Jacob, *op. cit.*, pp. 12, 16f., 26, 34-39. The species of grain eaten by men since primitive times are six: millet, oats, barley and wheat in the earliest period; rye since the late classic period; and maize since the discovery of America. Raised bread can only be prepared from wheat and from rye (which is a comparatively late discovery). Bread is a product baked in a properly constructed oven from a dough that has been raised by yeast or some other leavening agent. The menu of prehistoric man and even of civilized peoples consisted for a very long time of porridge and flat breads (cf. Pliny, *Nat. History* XVIII, 19: *Pulte autem, non pane, vixisse longo tempore Romanos manifestum*), and but for the discovery of bread and the invention of the oven to bake it in (for the new product could not be baked, as flat breads had been, in the coals of a fire) by the Egyptians, they would never have known bread.

The pastoral Hebrews did not bake bread, for they had no ovens like the Egyptians but parched their grain or set flat cakes to

bake between layers of slowly burning fuel.<sup>18</sup> The result was a flat cake like the *pantis subcineraria* of Roman armies on the march. It was only through intercourse with the Egyptians during their sojourn in Goshen that the Hebrews learnt to bake bread and they would not have done so had they remained shepherds.

<sup>18</sup> Athenaeus, IV, 14, p. 137.

<sup>19</sup> *Leviticus*, XXIII, 10-16.

<sup>20</sup> *Leviticus*, XXIII, 15-18.

<sup>21</sup> *Exodus*, XXIII, 16, and XXXIV, 22.

<sup>22</sup> Hastings, *Enc. Rel. and Eth.*, Vol. V, Festivals and Feasts (Hebrew), p. 865 n.

<sup>23</sup> *Leviticus*, XXXIII, 17.

<sup>24</sup> Hastings, *loc. cit.*

<sup>25</sup> This fast was interpreted as a sign of gratitude for the miraculous escape of the first-born Israelites at the time when all the first-born males in Egypt were slain. It can hardly be the truth and the story is merely another etiological explanation of an ancient custom.

<sup>26</sup> H. V. Morton, *In the Steps of the Master*, London, 1949, p. 342. Cf. the 'whited sepulchres' of the New Testament, *Matthew*, XXIII, 27.

<sup>27</sup> Cf. Frazer, *Golden Bough*, Vol. II, p. 75 (abridged edn., p. 485).

<sup>28</sup> *Exodus*, XXIII, 18, XXXIV, 25. Cf. H. E. Jacob, *op. cit.*, pp. 37f.

<sup>29</sup> J. Harrison, *op. cit.*, pp. 84ff.

<sup>30</sup> *Ibidem*, p. 87. For the discovery of fermented bread by the Egyptians see H. E. Jacob, *op. cit.*, pp. 12, 16f., 26, 34-39.

<sup>31</sup> J. Harrison, *op. cit.*, p. 87.

<sup>32</sup> Frazer, *The Scapegoat*, London, 1920, p. 408. *Ibidem*, p. 394, for the close correspondence between the Roman festival of the Saturnalia, the Christian Carnival and Purim, and p. 413 for the view that the Saturnalia was originally held in March, i.e. in the spring.

<sup>33</sup> *Esther*, III, 7.

<sup>34</sup> Cf. Frazer, *op. cit.*, p. 360. The majority of critics assign the book of Esther to the fourth or third century B.C., i.e. to the Persian period.

<sup>35</sup> *Ibidem*, p. 36.

<sup>36</sup> John, XVIII, 39; cf. Matt., XXVII, 15; Mark, XV, 9; Luke, XXIII, 17.

<sup>37</sup> Langdon, *op. cit.*, pp. 131f.

<sup>38</sup> *Ibidem*, pp. 142f.

<sup>39</sup> Frazer, *The Scapegoat*, pp. 419ff.

## ROYAL ANTHROPOLOGICAL INSTITUTE PROCEEDINGS

**Mixtec Codical Art.** By Dr. Philip Dark, West African Institute of Social and Economic Research, Ibadan. Summary of a communication to the Institute, 16 February, 1956

**38**

The Mixtecs today number some half million. They live principally in the western half of the state of Oaxaca, with small numbers over the border in the neighbouring states of Guerrero and Puebla. Mixtec peoples have lived in this area for centuries, their earliest remains pointing to an occupation during an archaic horizon coincident with the first period of the occupation of Monte Alban.

Though considerable information is available on the Aztec and Mayan civilizations, in Central Mexico there is a large gap of about 500 years up to the time of the Conquest where various collections of data do not seem to fit into a reasonable pattern. To fill the gap, Vaillant postulated a culture having a complex of traits (the Mixteca-Puebla complex), including codices, which originated in the Mixteca-Puebla area and existed from A.D. 1100-1300. Caso, through his work on Zapotec monuments, was led to attempt to solve the manuscript writings of the Mixtecs, which, he maintains, recount their history as far back as the seventh century A.D. Whoever the purveyors of this complex,

which spread so widely—as far as Sinaloa to the north and Salvador to the south—may have been, a key to its understanding and of this pre-Conquest period in Central Mexico seems to lie with the Mixtec codices, if they could be read.

There are some 19 or more examples of Mixtec codical art which have survived; 11 were painted before the Conquest. Of these there are two principal groups: the Borgia and the Zouche-Nuttall. The former appear to be concerned with mythological, calendrical and astronomical matter, the latter with historical and political accounts of important people and their deeds. The principal workers on this last group have been Cooper-Clark, Long, Spinden and Caso. Caso, working backward from ascertainable dates in the sixteenth century, has recounted the genealogies of some of the codices. Although Christian equivalents for Mixtec dates in the sixteenth century are ascertainable, it is not known what sign designates the beginning or end of a Calendar Round—the 52-year-cycle—and the date signs in one cycle are not distinguishable from those of another.

In order to obtain the maximum degree of objectivity possible in analysing the content of these codices it was felt that two requirements must be met: first, the phenomena observed must

be reduced to symbols that could be more easily handled in analysis and, second, all elements must be controlled so that hypotheses as to meaning could be applied consistently, not only for a codex but cross-codically as well. Previous approaches do not seem to have attempted to obtain meaning in terms of the patterning of elements nor do they appear consistently to have maintained hypotheses as to meaning in the course of analysis of the codices. The method devised is called the Ideographic-iconographical Method. It consists of two systems: the Ideographic Method and the Aspect Sheet System.

In the Ideographic Method two steps are taken. First, a few simple specially designed signs are employed as substitutes for the pictographic forms of people and places in the codices and are plotted out in a representation of the codex. These signs are ideographic, substituting for visual appearances without signifying intrinsic meaning. In the second step, a small number of letters and numbers are made to represent the ideographic analysis of the codex. This resulted in a patterning of letters and numbers suggesting that certain relationships were present between the forms and constituting a pattern. To discover the meaning of such patterns, meanings for certain types of patterns are postulated in terms of the identities of the pictographic forms, i.e. the names of the personages depicted in the codices. After several modifications three assumptions as to the meaning of the ideographic patterns were arrived at and applied to the pictographs of Codex Selden. This resulted in genealogies being obtained for the codex.

As a result of the application of the Ideographic Method to Codex Selden it was found, briefly, that all the people in the last 10 pages of the codex were related, resulting in 11 generations. Caso's dates for the second half of Selden are A.D. 1292-1546, a period of 254 years. Eleven generations at 20 years per generation gives 220 years. No attention was paid to dates. Three simple assumptions were made in the analysis, hence an appeal to the evidence is simple.

### **Survivals from the Second Millennium B.C. in Modern Turkey. By G. R. H. Wright, architect, and J. Carswell, draughtsman, British Institute of Archaeology, Ankara. With seven text figures**

**39** During the excavations conducted by the British Institute of Archaeology at Ankara on the mound of Beycesultan, near the small town of Çivril in western Turkey, it became apparent that the excavation house, together with the ox carts of the vicinity, represented survivals from the second millennium B.C., the period under investigation on the mound. The site of Beycesultan is close to the modern village of Menteş, situated in Phrygia near one of the old courses of the Meander River, which in classical times was probably called the Glaukos.

*The Traditional Timber-Mud-Brick House in Menteş Village.* These houses are of two floors and are constructed on a rectangular ground plan with heavy buttress walls on three sides, the fourth side being wholly or partially open on the first-storey level to form a similar feature to the open first floor of an Elizabethan coaching inn. Here an external staircase provides the only means of access to the upper floor.

The process of construction is as follows. Foundation trenches are laid out accurately, correspondingly larger for the three external buttress walls than for the partition walls (e.g. 80 centimetres as against 18 centimetres). A complete stone substructure

is then laid to a height of e.g. 50 centimetres above ground level. For the external walls strong timbers are set up at suitable intervals to form the main vertical members of the structure—they are of a height sufficient to compass the two storeys. These walls are then built up in this manner. Along both the outer and inner edges of the stone substructure, stringer beams are laid which form at the same time a levelling course and bases for twin half-timbered frames. Between these frames the walls are then carried up in mud brick and are given a mud plaster finish. The partition walls are also built up to first-floor height in mud brick laid between single frames of slighter timber and capped by stringer beams.

At this stage heavy cross beams are laid between two buttress walls. These cross beams are very grand, in some instances well over 10 metres long. Athwart the cross beams numerous pole rafters are placed which form the supports for rush matting, on which is set a layer of fine earth, comprising a ceiling-floor complex.

The upper stage of the building is identical, with the exception that a thicker layer of rushes and mud overlies the matting to form a flat roof with projecting eaves.

The structural framework thus described is the same as that used in the Great Burnt Palace, c. 1400 B.C., at present being excavated by the expedition on the neighbouring mound of Beycesultan. So similar are the details, that the writer and the

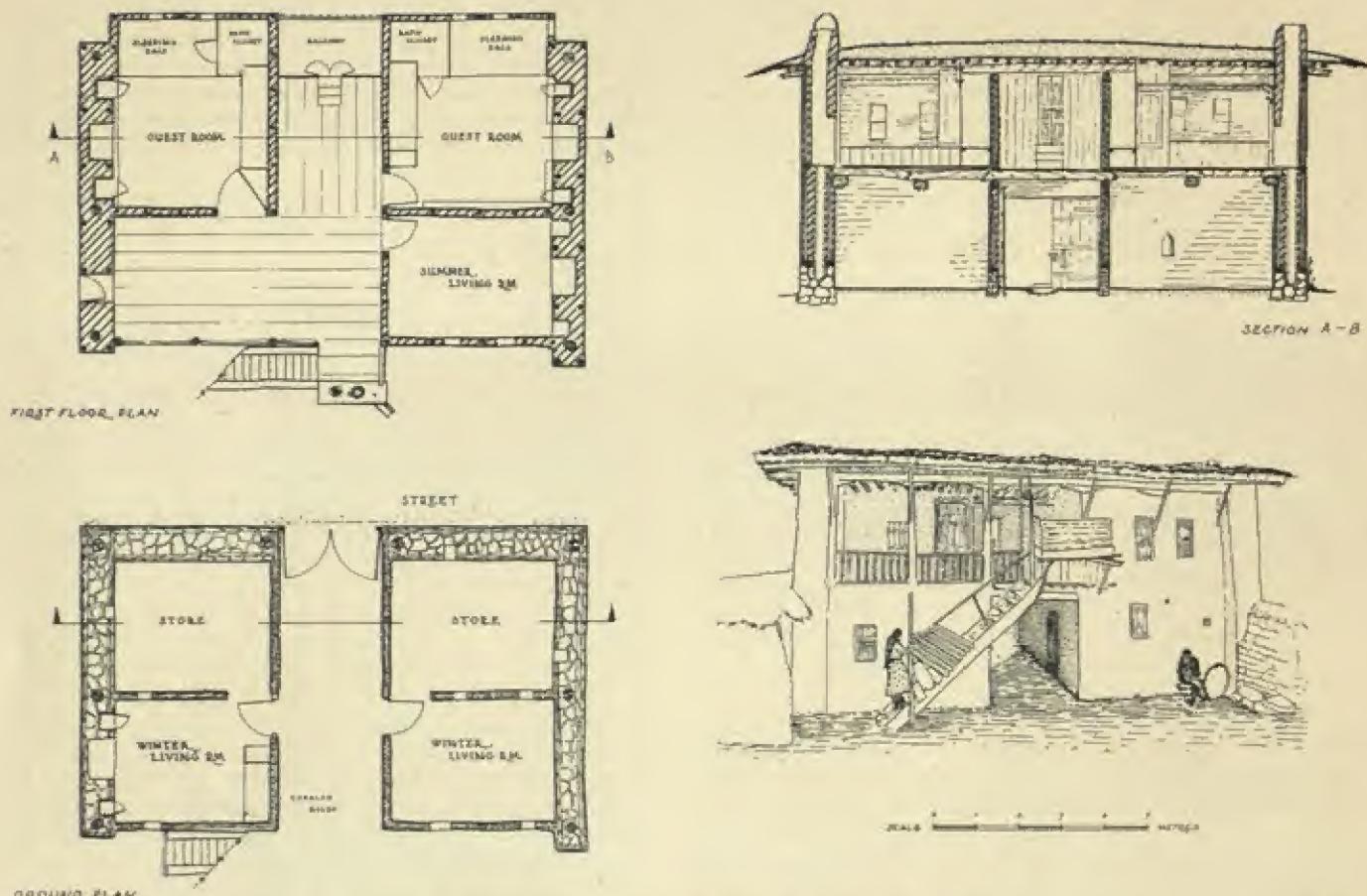


FIG. 1. EXCAVATION HOUSE IN MENTES VILLAGE, TURKEY

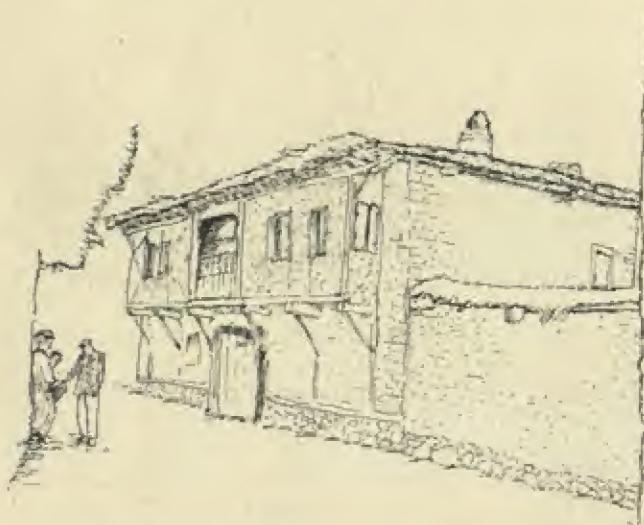


FIG. 2. ANATOLIAN HOUSE: STREET FRONT

Director of the excavations spent some ten minutes of pertinent discussion before it was realized that three and a half thousand years separated the two subjects concerned.

In this as in other aspects of Anatolian life, Phrygian, Hellenistic, Roman, Byzantine and Turkish elements have come and gone, producing individual variations, but it is the old tradition

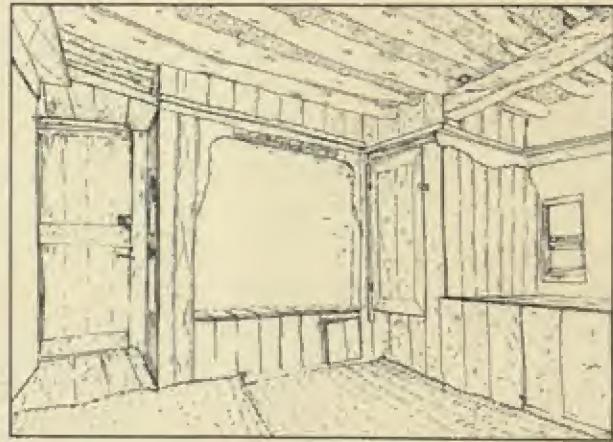


FIG. 3. ANATOLIAN HOUSE: GUEST ROOM

*Left to right: door, cupboard, bath closet and sleeping bunks*

that has remained the most firmly rooted. For example, at the present day a few houses are being built in Mentes village with the conventional pitched roof of Western Europe. The tiles of these roofs are a symbol, but the upper rooms lack a structural ceiling, since a ceiling can only be envisaged as the underside of a floor or roof.

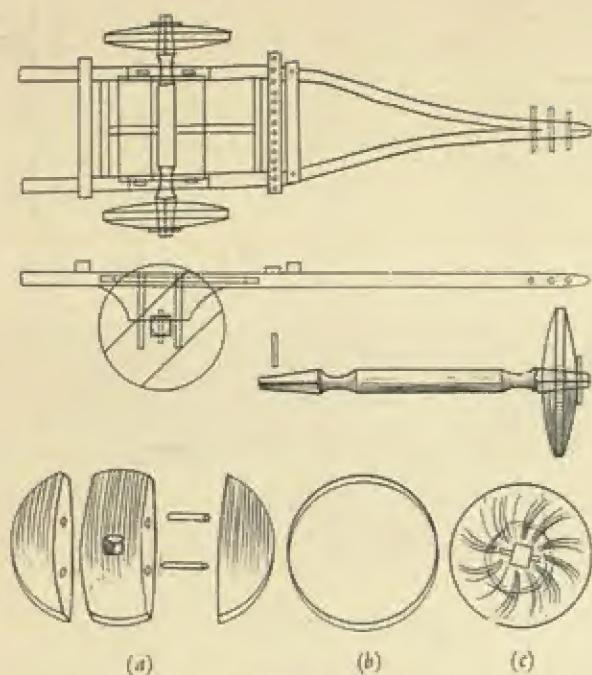


FIG. 4. ANATOLIAN OX CART

This cart is the simplest of all. The axle is a rough log with square tapered ends pinned into the wheels (a) which are made from three solid blocks dowelled together and bound with an iron rim (b). Both axle and wheels revolve together and the chassis rests on two grooves. The wheels are decorated with a scored pattern (c).

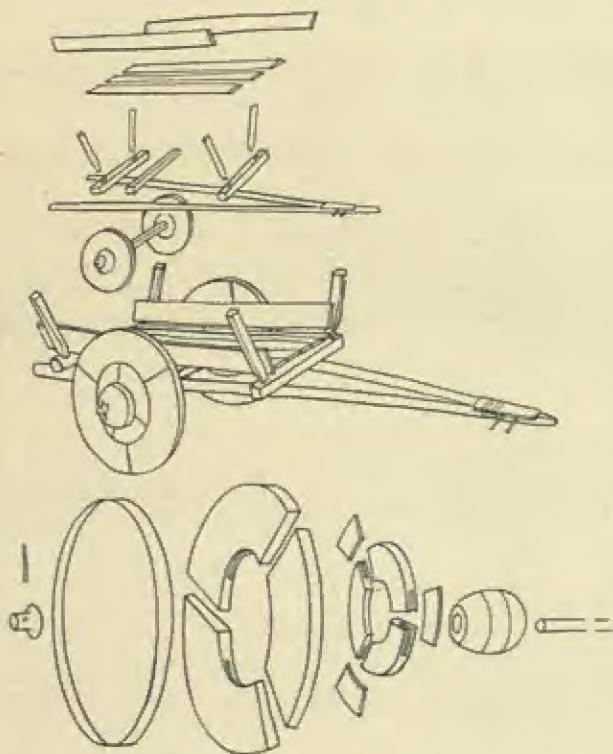


FIG. 5. ANATOLIAN OX CART

It has a square box-like framework useful for almost any purpose. It is rather less primitive than fig. 4 as it has an iron axle, which does not turn. The wheels are more complicated, being made of six wooden sections flanged together and a solid wooden hub which turns on the end of the axle.

*Anatolian Ox Carts.* These carts are by no means the only sort of wheeled transport used in the villages.

For longer journeys a four-wheeled *araba* or simple horse-drawn cart is much more likely to be used, and some farmers already own a jeep. But for rougher types of work in the fields,

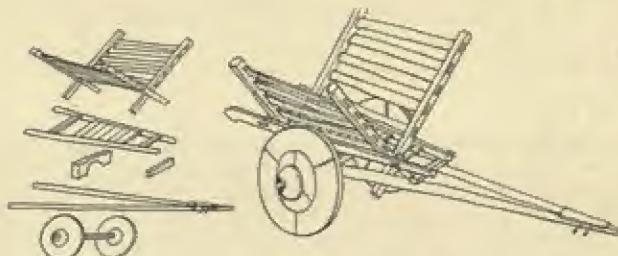


FIG. 6. ANATOLIAN OX CART

The cart is made of seven distinct parts, and is a simple variant of fig. 5. The uppermost X-shaped structure for carrying large and cumbersome loads, such as straw, is easily removed, leaving a level platform.

wooden ox carts have a looseness of construction which gives them a flexibility particularly suited to the uneven terrain.

Like the wooden frame houses they are traditional; they have an historical parallel in the twelfth century B.C. reliefs at Medinet Habu in Egypt, where northern invaders are shown riding in similar ox carts with solid wooden wheels.

They are made by the village carpenter from local timber. Most of the rough shaping is done with an adze, and all the

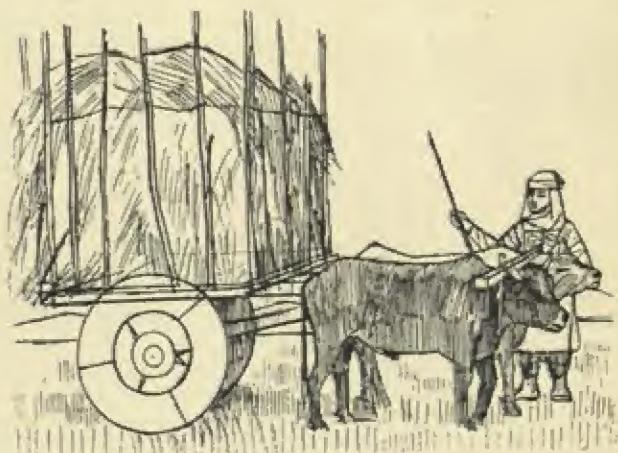


FIG. 7. ANATOLIAN OX CART

The carts at harvest time have a long rectangular shutter placed on the chassis. Its four sides are all pierced and have long staves driven in which stand up vertically to keep the load in place. To unload is a simple matter of taking out the back row of staves and tipping the cart backwards.

joints are dowelled. Although only four examples are mentioned below, there are many individual types of cart ranging from district to district. At first glance all seem to be quite different, but closer examination shows that they always have two common features: a pair of solid wooden wheels, and a simple fork-shaped chassis raised off the axle by two wooden blocks. The superstructure is adapted to the task at hand and changes all the year round. The carts are invariably hauled by a yoke of oxen (fig. 7).

## REVIEWS

## GENERAL

**The Nature and Function of Priesthood.** By E. O. James. London (Thames & Hudson), 1955. Pp. 336. Price £1 5s.

**40** Although Professor James's latest work is written for the wider educated public it is not without all interest for social anthropologists. It is useful to have the more recent field material brought together and compared with the older literary evidence. Such books as this provide the opportunity for social anthropology to step back, as it were, and view itself in the context of another discipline.

The subject is allowed to evolve from a consideration of 'magico-religious practitioners' in primitive society through chapters distinguishing seers, diviners and prophets from priests and goes on to consider the priesthood in relation to kingship, sacrifice, absolution, sacred learning and jurisdiction. The material is drawn from the ancient world of East and West, pre- and post-Reformation Europe, and primitive material is cited from the works of Radcliffe-Brown, Rattray, Malinowski, Seligman, Evans-Pritchard, Layard, Firth and Meek.

In a book of so wide a scope it is not to be expected that all sections will be equally rewarding and Professor James is at his best where he is at home in the post-Reformation period. It is perhaps unfair to single out for adverse comment the section on Hinduism on which the reviewer is qualified to speak, but there the discussion seems very thin compared with the more detailed work of French and German writers which is not cited. Again it is perhaps surprising that Radhakrishnan's views of religion in ancient India should be quoted with such authority since this great apologist of Neo-Hinduism is naturally concerned to stress the mystical and ethical aspects of his belief rather than the sacrificial ones.

But in this field no other English scholar of recent years has shown himself capable of welding together such a mass of material. Others will be able to refine upon the analysis of particular concepts now that the broad outline of the problem has been drawn.

D. F. POCOCK

**The Origins of Art.** By Gene Weltfish. New York (Bobbs-Merrill), 1953. Pp. 300 (including 99 plates). Price \$4.50

**41** Dr. Weltfish is a leading authority on American Indian basketry. Her technical appendix (pp. 243-264 of this book), which describes the basket-making techniques of North American Indians, proves this, although the value of the drawings which illustrate it would be greater if they were properly captioned, or even referred to in the text. Even after careful study, it is impossible, for example, to see the point of Plate LXXXVIII, figs. 3 and 6.

The rest of the book is disappointing. In the first place no real attempt is made to fulfil the promise of the title. A brief and extremely misleading account of Palaeolithic art is found in Chapter I, a general introduction of six pages. Presumably it is because this book is intended to have popular appeal as well as scientific value that Dr. Weltfish neglects to use the names of prehistoric European cultures, with the convenient effect of telescoping the Lower and Upper Palaeolithic, so that the maker of developed Acheulian hand-axes appears to be the manufacturer of Aurignacian figurines:

'But the man was not satisfied to have just a sharp edge. He wanted a point on his tool in order to break the bones and to dig out the marrow. And then for nearly a thousand centuries he worked to make a tool with a good point and clean sharp edges.'

'At last he achieved success. His tool was a masterpiece of skill in the working of flint. But along with the mastery of his skill he had also achieved a new kind of satisfaction. The tool was a much better cutting instrument than he had ever made before, but this was not

its only value. It was smooth and symmetrical in shape, and beautiful to look at and to touch.'

'Then he picked up a small limestone pebble and sculptured the shape that was most beautiful to him—the shape of a woman. And on limestone slabs that he placed around the walls of his rock shelter, he carved the forms of women in bas relief' (p. 225: recapitulation of 'The Origins of Art in Very Remote Times As Presented in This Book.'). I leave unremarked the other implications of this passage.

She illustrates examples of the Key pattern from Kiev (final Palaeolithic), Danubian II, Egypt (c. 1500 B.C.) and New Mexico (c. A.D. 1000) and following Semper, makes a plausible case for the origin of this pattern in twilled matting of a type which she describes from Brazil. She shows the derivation in America from similar matting, of other motifs—a concentric diamond pattern and the diagonal step. However, the derivation of the variant of the former pattern shown on Plate VI is not convincing.

Chapter IV shows very well the technical processes which produce patterns in basket-making among the Bakairi of the Amazon, but it is marred by such nonsense as the three paragraphs beginning with the last on p. 55, where she elaborates the great differences in the four parts of a pattern, and then concludes: '(Actually, each of the four sectors was twill woven in exactly the same way . . .)' (p. 56)—which is obvious to anyone except Dr. Weltfish.

Chapter V discusses the development of Mimbres pottery decoration, seeing it as an elaboration of textile designs (but she admits on p. 90 that her choice is highly selective—that there are patterns not so derived). Here, when unsure of the interpretation of the patterns, she consulted her twenty-year-old art-student daughter, who supplied highly subjective interpretations of geometric patterns which are quite out of place in a supposedly ethnological treatise. For example the five pots on Plate XLVIII are claimed to represent 'atmospheric qualities,' e.g. 'transparency.' Her own contributions are on the same level—one pattern represents 'the pulsations of birth' (Plate XLIV, 3).

Chapter VI derives the rest of North American art from birch-bark containers and parfleches. Her case rests on the structural emphasis of parfleche patterns, producing geometrical designs based on the shape of the flap, and the angularity of folded rawhide. It is declared in passing, without reference to supporting evidence, that the American seed-gatherers invented agriculture (p. 129), and that the Pomo invented lattice twining (p. 262) as if it were found nowhere else in the world.

There are 99 pages of line illustrations, most of which are clear and excellent, but the lettering of the maps and sections is abominable. Some of the illustrations when borrowed from other writers could have been better chosen, e.g. Plate LVI, 3, is a poor example of structural emphasis.

The book is irritatingly arranged. Most of the time one needs to keep five fingers in the book at once in order not to miss the connexion between the text, the illustrations, the captions, the notes, and the sources of the illustrations.

Dr. Weltfish tells us (p. 12) that over 25 years and 100,000 dollars' worth of research and travel have been absorbed into this book, and (p. 13) that she has held back this material to publish it in this form 'instead of following the usual practice . . . of publishing a series of technical articles in obscure scientific journals.' (e.g. the *American Anthropologist*, where she has previously published?). In this way she has deprived herself of constructive criticism which would have helped to make this book less unworthy of its title. Two papers of 'Studies of American basketry techniques and their influence on other crafts' would have done the subject—and the author—more good.

FRANK WILLETT

## AFRICA

**Ethnographic Survey of Africa: North-Eastern Africa, Part II; The Galla of Ethiopia; The Kingdoms of Kafa and Janjero.** By G. W. B. Huntingford. London (Intern. Afr. Inst.), 1955. Pp. 156. Price 16s.

This volume extends the Survey to the Galla and the Sidama of

Ethiopia. The information is arranged according to the general pattern, with detailed indices, and is an admirable summary illustrated by several sketch maps and a few line drawings. The bibliographies look promising and no doubt omit few references; but, as the author emphasizes, a general review of the region is difficult

because the books available, though the earliest reaches back 350 years, describe different tribes at different times, and most of the primary sources are now more than fifty years old. Thus we have no eye-witness of a full eight-year operation of the *gada*—a kind of age-set system—which is the basic institution of Galla society.

A disappointing lacuna in the information available—though not one that Mr. Huntingford dwells on—concerns these tribes' relations with the Abyssinians. Conflict with the Abyssinians has provided the main events in the history of the Galla ever since their first savage deployment 400 years ago, if not in that of the Sidama, and contact with the Abyssinians must be the main factor in social and cultural change. Alternately conquering and conquered, the Galla now occupy a good third of Menelik's empire and are the biggest if not the politically dominant group in it. What are their land rights under the Abyssinian régime? (Incidentally, a *gebar* in Abyssinia is not a slave.) What is the status of Abyssinians in Galla districts like Wollo? What is the balance of cultural exchange in the capital province of Shoa? Similar unanswered questions are posed by all the tribal lands that enclose Ethiopia's Abyssinian heart.

D. J. DUNCANSON

**Jinja Transformed.** By Cyril and Rhona Sofer. E. Af. Inst. Soc. Res., Kampala. E. Af. Stud. No. 4, 1955. Pp. xi, 120. Map, 8 photographs. Price 16s. 6d.

**43**

Jinja lies on the east bank of the new-born Nile. For many years before the extension of the Uganda Railway from Kenya it was the southern terminus of a local line, which makes the omission of any statistics about railway workers all the more remarkable in a book which deals largely with statistics. This popula-

tion survey of a multi-racial township is, however, a painstaking and, so far as I can judge, careful production. The town is in Busoga, but the Soga form a minority in the African population; and since many of them have their own homes and local interests in the country round Jinja, they seem to be the least effective tribal element in the structure of the township. Moreover, owing to the impermanent nature of the population, only about half the adult male Africans qualify for a vote in the election of township councils, and there is in consequence little interest in these matters.

Although the occupations of Africans are analysed in several tables, certain of them need to be broken down still further to give a complete picture; none of the tables, for example, give any clue as to the numbers employed on the railway, some of whom must surely spend part of their time at Jinja. Another table brings out the tendency to frequent changes of job—a phenomenon not unknown elsewhere in East Africa; and the number of unskilled labourers, due mainly, it would seem, to lack of education, is still very high among African workers in Jinja. An interesting point is that there are signs of the emergence of a new class structure based on occupation rather than on tribal affinity. Among the Asians, the largest occupation group is that of the traders, with builders next, and then, a long way back, workers in transport, manufactures, education, and law. The order of frequency in occupations of the European population is building, public services, and commerce. The size of the building group is said to be due mainly to the Owen Falls works; and many of the European workers are foreigners. Part of the book deals with incomes, standards of living, and the household and kinship situation. Though not exciting reading, there is good deal of solid, useful matter in the book; it would have been more useful with an index.

G. W. B. HUNTINGFORD

## EUROPE

**Il Mediterraneo, L'Europa, L'Italia, durante la Preistoria.** By P. Lariosa Zambotti, Turin (Società Editrice Internazionale), 1954. Pp. viii, 286. Price 1500 lire

**44**

This is a sequel to the same author's *Origini e diffusione della civiltà* (1947), in which she gave her view of the spread from the Near East to Europe of the earliest neolithic village civilization, which was agricultural, democratic and matriarchal. It is contended here that this culture in its diffusion passed by Anatolia, where there grew up a civilization of a different character, pastoral, aristocratic and patriarchal. The whole subsequent story of European prehistory is understood in terms of the spread, evolution and interaction of these two civilizations. That from Anatolia, for example, radiated to Spain as the Conca d'Oro culture, to the middle Danube, where it influenced the rise of the Hittites, Achaeans and Veneti, and to the Caucasus-Caspian area, whence came the Kuban civilization, the Beaker folk, and later the *sarmat*-speaking Phrygians and Thracians.

The premises and methods of the author recall those of the ethnologists of the classical Vienna school, and like them she exposes herself to the criticisms of relying too exclusively on ethnic migrations to explain the diffusion of culture, and of too readily recognizing distinct civilizations which travel unchanged and intact. The general thesis is too simple to carry conviction, and proceeds through some dangerous assumptions: it is unlikely, for instance, that a pastoral economy was first practised in Anatolia, and the Hittites almost certainly did not come from Europe. But everywhere a clear and fresh mind is seen at work, and some of the comparisons, such as that between the treasures of Alaca Hüyük, of the Kuban burials, and of Troy II/III, are very original and stimulating. The author brings into her scheme the most recent excavations and writings, and also includes maps and illustrations which are well chosen and often unfamiliar. Unfortunately there is no index.

W. C. BRICE

**The Physical Anthropology of Ireland.** By E. A. Hooton and C. W. Dupertuis (with a section on the Irish West Coast Females by Helen Dawson). Peabody Mus. Pap., Vol. XXX, Nos. 1 & 2. Cambridge, Mass., 1955. Pp. ix, 448, 47 plates, 38 maps. Price \$10 (paper), \$13 (cloth)

**45**

characters: namely, hair colour, eye colour, cephalic index, nasal index and stature. Pages 141-3 deal with these 'sorting criteria,' as the authors call them. They are presented obscurely, although in fact they are logically sound. Nine-tenths of the male Irish fit into four of the types recognized. Of these, 36 per cent. are brachycephalic, equally divided between 'Dinarics' and 'Nordic Alpines'; these are separated by the nasal index. Some 54 per cent. are almost equally divided between 'Keltics' and 'Nordic Mediterraneans,' the latter being slightly more abundant. The 'Alpine' type was nowhere found, and 'Pure Nordics' and 'Pure Mediterraneans' together amounted to less than 1 per cent. of the total. The authors rightly stress the low incidence of dolichocephaly and the relatively high occurrence of brachycephaly in Ireland. The Catholic, Anglican (Church of Ireland) and Presbyterian communions are treated separately to begin with; their particular statistics are then compared with a view to assessing the influence of the Elizabethan, Jacobean, Cromwellian and Williamite plantations upon the older stocks. The Scottish element is thus shown to be a distinct, markedly Mediterranean, component of the people of north-east Ulster. The politicians will no doubt find in this part a plateful of bones to growl over.

The maps of relative distribution of the ethnical types show undoubtedly significant differences between the different parts of Ireland. An attempt is made to correlate these differences with the results of modern studies upon the linguistics, mythology, legends and history of the island. The delay in publication allowed all the relevant material in this field to be read and considered, including the material published by the late Professor T. Ó Ráthaille, the most notable linguistic authority. The authors find that their anthropometric study accords with his conclusions upon the prehistoric colonizations of Ireland more nearly than with those of earlier scholars. In this the reviewer agrees with the authors. He is conversant with the relevant linguistic and historical literature, has examined most of the sparse skeletal material cited in the present work, and had himself carried out a 'pilot' anthropometric survey on Irishmen from all parts of Ireland in 1923 (during the partial

demobilization of the Irish Army). From all this he had concluded by 1940, that the idea of four successive and distinguishable waves of colonization—Pritenic, Erannian (*Fir Bolg*), Laginian and Goidelic—was more likely to be true than false. In particular, he had noted the peculiar anthropometric characters of the folk of Westmeath, ancient and modern, and the tradition associating this region with the *Galioin* (part of the Laginians). Dupertuis's maps furnish a remarkable verification of the special ethnological nature of this region: it is a veritable island of statistical differentiation from its surroundings. Space does not permit of an account of the many other remarkable ways in which our authors have substantiated the age-old accounts of regional difference implicit in the works of native historians. The publication of this survey marks a real advance in our knowledge of the Irish people in space and time.

The study of West Coast Females by Professor Dawson breaks new ground. It raises too many questions, however, to be adequately discussed here.

Apart from the numerical faults already mentioned, there are only two matters calling for adverse criticism. First, the numerous sections headed *Sociology* should have been omitted. The methods by which subjects were obtained were not such as to yield a representative sociological sample in any area at the time of the investigation, particularly in Ulster. To group 'tinkers' with 'hired labourers' is a major error in Irish sociology; and erroneous citations of governmental and ecclesiastical titles do not increase one's respect for the authors' assertions about social structure. Second, the 'historical sketch' of the survey by Dr. Dupertuis should have been severely edited. It is a well-meant attempt at thanks to his Irish hosts and helpers, but it is stuffed with *gaffes* which even those Irishmen who are tolerant of American *gaucherie* will shudder at. This is a pity, for the book should find a place in all Irish libraries, popular as well as learned.

The printing, maps, photographs and tablature are excellent. There appear to be only two printer's errors, both minor. The work can be heartily recommended to all anthropological libraries.

M. A. MACCONAILL

## CORRESPONDENCE

### For a New Definition of Marriage. Cf. MAN, 1955, 199

**46** SIR,—I heartily agree with Dr. E. R. Leach when he writes that 'the *Notes and Queries* definition of marriage is too limited.' As far as I can see, that definition contains today only those 'distinguishable classes of rights' which Dr. Leach has on his list under headings *A* and *B* (to establish the legal father of a woman's children; to establish the legal mother of a man's children), and this may be because in our own society it is marriage that establishes legitimacy in the offspring. In other societies (such as the Todas, for instance) marriage and legitimacy of the children can be looked upon as two different and separate concepts, and it may be necessary to go through a ceremony of legitimization of the offspring (the *Toda purisupimi* ceremony) in order to establish who is the legal father, because marriage rites are insufficient in themselves to do this.

Thus, the status of husband and of father should not be looked upon as necessarily identical in all societies. It is no doubt because these very often erroneously are so considered that we get definitions of marriage couched in terms of paternity (Fisher finds it necessary for each of the husbands in turn to be considered the social fathers of the common wife's successive children before he can agree to polyandry being polygamous). That is why I am sure that Dr. Leach is perfectly right when he says that 'in no single society can marriage serve to establish all these types of right simultaneously; nor is there any one of these rights which is invariably established by marriage in every known society.'

For this reason, I have proposed that marriage be redefined as follows: *The socially recognized assumption by man and woman of the kinship status of husband and wife.* It is then up to the anthropologist to seek out and list which are the particular rights and obligations associated with this kinship status in the society which he is describing. And I am sure that he will then discover that, as Dr. Leach so

aptly has it, 'the institutions commonly described as marriage do not all have the same legal and social concomitants.'

Kalimpong, West Bengal PETER, Prince of Greece and Denmark

### A Welsh Wassail-Bowl. Cf. MAN, 1955, 88

**47** SIR,—In my description of this bowl made in Ewenny I stated that 'the flat top is inscribed *Spring* and *Langan*' interpreting the latter as Langan, a village not far distant from Ewenny. I suggested that 'a figure, obviously that of Spring, has been broken off,' and ventured a folkloristic explanation of the decoration.

A recent reference in *Country Life* (26 January, 1956, pp. 150-1) leads me to believe that the folklore of this bowl-lid is, in part, much less traditional than I had supposed. It appears that in 1824 a certain Tom Winter, nicknamed Spring, fought a boxing contest at Chichester with an Irishman named Langan, the contest lasting one hour and forty-nine minutes. Subsequently Staffordshire jugs depicting the fight were made and placed on the market. Although the date of the Ewenny bowl is ten years later (1834) than the fight, it seems most probable that the figure missing from the lid must have been a representation of Tom Spring, the other mutilated figure being of the boxer Langan. Nothing on the bowl-lid leads one to suppose that its missing crest commemorated a famous boxing-contest, but the presence of the two names *Spring* and *Langan* together on the lid makes it unlikely that the scene could be any other than the contest referred to. Why a Chichester boxing-match should be commemorated on a Welsh wassail-bowl is itself a mystery.

I record this correction here to indicate how dangerous the slightest theorizing can be unless the theorist's knowledge (even of boxing history, of which I know nothing) is encyclopaedic!

St. Fagans, Cardiff

IORWERTH C. PEATE





(a) Son of Henry of Kataima, showing a neck sore which caused him to seek the aid of a taling genak, a 'possessor of blowing'



(b) A food stone charm (Kiali Təbu), which, accompanied by blowing, may be used to cure fever. The stone is marked X



(c) Francis of Tagaikapai, an Akawaio shaman, setting out to visit a patient who has asked him to consult the spirits on his behalf



(d) Francis of Tagaikapai drinking tobacco juice before conducting a spirit séance

#### RITUAL BLOWING AMONG THE AKAWAIO

Photographs: Audrey J. Butt, 1950-1951

# RITUAL BLOWING

TALING—A CAUSATION AND CURE OF ILLNESS AMONG THE AKAWAOI\*

by

DR. AUDREY J. BUTT

Pitt Rivers Museum, Oxford

**48** *Taling* (ritual blowing) is one of the most important causes of sickness and death in the Akawaio system of beliefs. At the same time, it is regarded as an effective means of curing illness and of achieving or preventing certain specific aims.

The word *taling* means 'blowing.' It refers to a special procedure, not to everyday breathing, for different words are used in these connexions: *Ura yelubalu* (I breathe), is the word for regular inhaling and exhaling of breath in the normal process of breathing; *Ura ya etalingba* (I blow *taling*), refers to a particular mode of breathing or blowing. This second, special mode of breathing is used only on certain occasions and in association with certain specific acts and intentions.

## Method of Blowing

People who 'blow' do so by forcing their breath, either through the mouth or down the nose, in short sharp gusts. The blower also utters a charm, or a specially worded wish or command, either silently within himself or quickly, in a subdued tone of voice. These *taling* words are uttered at the end of the blowing or between the gusts of breath, or both. The important thing, for successful blowing, is that there should be both an ejection of breath and a projection of a specific wish or demand.

When a person blows he does so in the direction of the person or thing he wishes to affect. If rain clouds are being blown away a person will blow in the direction of these clouds; if a headache is to be banished the sufferer's head is blown upon; if a distant Indian is to be made sick the blowing is directed towards the region where he lives. Distance is of no account, for a person can blow effectively on someone many miles away, or on someone who is only a few feet or inches away.

Although distance in space is immaterial, distance in time is not. Blowing is believed to achieve success only when an issue appears undecided and a specific result is not already regarded as inevitable. As an example, I was told that if *Edodo*<sup>1</sup> is already approaching your house, blowing will not prevent his coming. If he is a long way off and is not actually coming, but you fear that he may come, then the right blowing will keep him away. *Taling* of this type is done when the tracks of a strange man are seen in the forest, or *Edodo* whistles are heard—when there is reason to believe that *Edodo* is contemplating an attack. If a person is ill *taling* may help him to get better, but not if his fate has already been determined and he is obviously on the point of death.

In other words, the Akawaio realize that blowing is no good when an issue is already certain in time, but it can be

\* With Plate D and two text figures. The substance of a communication to the Royal Anthropological Institute, 19 January, 1956

effective when a desired sequence of events may be instigated or when an undesirable sequence may be altered. For this reason, among others, blowing cannot be subjected to scientific tests for it is directed at the uncertainties of the present and future, and not at concrete realities of the present which are plain for all to see.

## The Theory of Blowing

The Akawaio maintain that they do not know how or why blowing achieves the required ends, but by gathering a few ideas here and there, from the more thinking individuals, one can obtain a certain insight into the problem.

I. *The Spirit in the Breath.* The Akawaio conceive of a close relationship, sometimes a complete identity, between a person's breath and his spirit. A spirit is said to be like the breeze and you feel it as it passes by. The *akwalupa*, the ghost spirit, is a breath spirit and the *akwatu*, the spirit or vitality of a living person, is contained in the breath.

I was told that in *taling*, the 'taling spirit' goes with the breath and does the work. However, there is no separate or special spirit called *taling*. When a person blows it is that person's own spirit or vitality which is projected in the breath and which is sent to perform certain work.

The Akawaio word for breath is *endabima*. This word is used for everyday breath (*yelubalu*) and for the breath which is blown in *taling* (*etalingba*). The fact that there is only one word for 'breath' but two words to describe its use according to circumstances justifies the assumption that it is only when the breath is employed during certain ritual forms, or when certain circumstances arise, that it has a spiritual significance and special power.

The association of the breath and spirit (*akwatu* in a living person and *akwalupa* in a dead person) is not an exact and coterminous one that we might postulate in a rigid scientific rule. I shall make this clearer by giving examples from other Akawaio beliefs concerning the nature of the spirit.

When a person sneezes violently they say 'your mother is calling you,' indicating that she is summoning the spirit to her. However, the sneezer still goes on breathing. A spirit pronouncement in a shaman's séance<sup>2</sup> asserted that the spirit of a dying boy had already left the body—but the boy was still breathing, as the shaman knew. The Akawaio say that they do not know exactly when the spirit of a dead man departs from the body—some time shortly after death is the most widely held belief. Thus, it is not correct to say that with a person's last breath his spirit flees, or the reverse, that when the spirit leaves, breathing necessarily ceases. On the other hand, they do assert that if the spirit leaves the body and fails to return after a time then death is certain—and breathing, of course, stops.

Everything points to the conclusion that although there is a close association between the spirit of a person and the breath, this correspondence is not one of inseparable identity. The spirit or vitality which is in the breath is thought to leave the body temporarily, without necessarily withdrawing all breath or impairing the natural function of breathing in any way. This concept is in harmony with general Akawaio thought on these matters, for they assert that when a shaman summons a person's spirit to a séance the owner does not feel anything and no one with him would know that there was any difference. The body is left intact and functioning as usual; only the spirit world and the shaman who is in contact with those disembodied spirits know what is happening. The effect on the person left without his spirit would be illness followed by death if this intangible but powerful principle delayed too long or failed to return to the body. Otherwise, he would feel no sensation at all.

In the act of blowing, therefore, a person is creating a special ritual situation in which he 'mobilizes' his vitality to achieve certain ends. The Akawaio explain this by saying that in blowing, a person detaches his own spirit from his body and sends it, in the breath with which it is associated, to perform certain tasks.

**II. Word Formulae.** If a blower is skilful he will utter special *taling* words when he blows. These take the form of a wish or command and they are considered very important for I was told that today most people have forgotten *taling* words and this is frequently the reason why they cannot be successful blowers. The part that these charm words play is not explained by the Akawaio. The fact that they have been forgotten in some cases indicates that they must be specifically worded and are not just haphazardly expressed wishes or commands to suit the occasion. My own impression is that since the energy for achieving a certain result is provided by the spirit (*akwahu*) of the speaker, this vitality which is sent forth in the short, sharp puffs of breath is deliberately willed and directed to its objective by the wish or command which is spoken at the same time. Possibly the specific wording compels a certain and automatic response and helps to bring about the desired end through some virtue in itself.

Both the act of blowing and the act of saying the necessary words are therefore required for really successful blowing. Occasionally, Akawaio do just blow and this, they believe, has some efficacy, although they say that through not knowing the words they 'cannot blow properly'. For example, an Akawaio father, about to start on a three-day journey, blew vigorously on the head of his baby son and on a little girl. This was to stop them crying, but he admitted that he did not say any words and that he could not blow properly—only some old people could do that!

**III. Tobacco Smoke.** In blowing, people sometimes use tobacco smoke. On the trail from Chinawieng<sup>3</sup> to Amokokopai<sup>4</sup> an old lady accompanying the party puffed at a cigarette in a special ritual manner. The smoke came out in sharply expelled little clouds, and at the same time she made a peculiar, soft whistling noise through the teeth. The

smoke was sent in every direction of the forest in turn for she was blowing away any evil spirits which might be following us on our three-day trek. The Akawaio say that many people smoke in this way, but personally I saw it done similarly on only one other occasion. The Guiana literature has many descriptions of this use of tobacco smoke.

Tobacco so employed is called *tamu*, a word which is also used for 'cigarette.' *Tamu* consists of tobacco leaves rolled in a layer of thin paperlike bark to form a huge cigar for smoking. Tobacco is used in blowing because it has an exceptionally strong and powerful spirit. Its strength 'carries' the *taling* and adds power to it in the journey to its destination and the activity entailed in bringing about the desired result.

**IV. Age.** Although success in blowing depends on whether a person has the right words—and these are inherited from close relatives—it is also believed that only old people can blow well. It is similarly the case with many skills, particularly those related to ritual matters. One Akawaio told me that if his mother were to die—she was an old woman experienced in ritual procedures and tribal lore—he did not know what he would do, as there would be no one with such effective skill to blow for his family. Experience and knowledge, which age brings, add to the 'spiritual stature' of a person, and it is the spirit or vitality which is one of the two important ingredients in *taling*.

Investigation into the theory of blowing therefore suggests the conclusion that it is a ritualized projection of a wish or command. It is believed that by means of the vitality contained in the blower's breath, the efficacy of a directing word formula, the carrying strength of tobacco and the wisdom of old age, a specific result can be obtained in circumstances when the outcome of present or future issues is uncertain. Only the first two ingredients, spirit vitality and word formula, are vital. The final two, tobacco smoke and the wisdom of old age, are important auxiliaries.

#### People Who Blow

Among the Akawaio anyone who knows how may blow, although some will not be as skilful as others at the practice, according to individual abilities, age and knowledge of particularly good words. Except for children and young people anybody and almost everybody practises blowing with, they believe, some degree of success. A striking instance of this occurred in a large hut occupied by several closely related families. A new-born child fell ill and was screaming because of soreness from an infected mouth. First, the old grandmother was called and she blew on the child vigorously. Then the father blew on its face. The baby was next taken to its grandfather who was head of the joint family group and then on to his wife and all his children—the brothers and sisters of the mother. By the end of the tour round the hut every adult had blown on the child to make it well, but an unmarried boy of about 16 years and children from 13 downwards were not included.

This widespread possession of the power of blowing, with greater or less effect, does not exist among the

Barama River Caribs of British Guiana, who, according to Gillin,<sup>5</sup> have a 'professional sorcerer' called *aremi emu*. He alone uses the blowing technique. The nearest Akawaio equivalent is the *taling genak*—the 'possessor of blowing'.<sup>6</sup> This title is usually given to those who are regarded as particularly effective blowers who have inherited or acquired a special knowledge, usually of one or two types of blowing only.

A person who has not got the right words himself may occasionally go to one who has a tribal reputation in this respect. Thus, Henry from Kataima, Mazaruni river group, went with his son to visit James of Waramadong, the Seventh Day Adventist village on the Kamarang River. He asked James to blow on the boy's neck where a large open sore persisted (Plate Da). Henry told me that some people, like James, have a knowledge of medicine which has been passed down to them from their fathers. As reputed blowers the use of their skill may be sought by others in the tribe.

In the majority of cases a person who wants someone to blow for him will go to his older relatives and ask them to do so. Usually they are people living in the same hut, or in the same settlement.

Blowing is not limited to humans for nature spirits are believed to blow most effectively. *Imawali*, the bush spirit, is said to blow on people at night, causing them to be sick. When the shaman summons spirits to the side of his sick patient, each spirit in turn approaches the hammock and blows to assist in the cure and show its good will. An angry spirit may cause headaches and all sorts of pains by blowing.

*Edodo*, the secret killer or sorcerer, sometimes blows and it is one of his many ways of making people sick. Usually *Edodo* is believed to blow to put someone to sleep or to make them ill so that he is able to creep upon them unawares and finish them off. His blowing is particularly powerful because it is able to send dogs to sleep to prevent them from barking and waking their owners while he is at work on his victim. *Edodo* rarely seems to rely on blowing alone to kill, as the evil *taling genak* does. This is shown by the fact that a person who dies of ordinary *taling* remains 'white'; if *Edodo* has been at work the corpse is 'black'; that is, it shows the characteristic blue marks said to be caused by his fingers.

#### Methods of Blowing Good and Bad

Apart from the diffusion of skill in blowing, the Akawaio differ from the Barama River Caribs in that they do not use pepper water or fall into trances. The most they do is to concentrate hard and appear, temporarily, somewhat detached and aloof during the mental effort involved. Thus, at the beginning of my research, when I lay prostrate in my hammock with stomach ache resulting from abrupt change of diet, an old woman came to me, looked sympathetic and blew vigorously on me eight or nine times and walked away again. Later, as I was walking out of the hut, the elderly wife of my Akawaio host blew on me twice. This is typical of the Akawaio informal approach in blowing!

As some of the preceding information has suggested, the

Akawaio believe that some people blow good and some blow bad. They say that evil people with evil intentions blow bad while good people, with good intentions, blow good. For example, Austin of Chinawieng blew on my eye which was inflamed and swollen from an infection caused by a certain small fly. I got better, I was told, because it was a good spirit in the blowing—Austin's spirit (*akwatu*) being a good one and Austin himself being a good man. It is because of the belief that some people blow bad that Akawaio frequently denied that they could blow at all when I asked them. Only when they knew me well would they admit to being able to blow, and when questioned about their ability, they always hastened to inform me that they themselves only knew how to blow good. Although I witnessed and frequently experienced personally, the practice of blowing to effect a cure, or to keep away some evil, I never saw anyone blowing sickness to another. This is because blowing evil is generally condemned and if attempted at all it is done in secret.

Like the Barama River Caribs, the Akawaio believe that a person who wishes to send sickness to another may do so



FIG. 1. SHAMAN'S SPIRIT STONE OF QUARTZ CRYSTAL

Said to contain *Imawali* (the forest spirit), and also the *Kawai* (tobacco spirit). About actual size, Pitt Rivers Museum. Photograph: K. H. H. Walters

by selecting an object and blowing it to the enemy. This penetrates the body and causes illness which may result in death if certain countermeasures are not taken in time. Objects which may be blown are many and varied, a knife, piece of wood, stone or any small, handy thing. The object is blown upon, frequently with tobacco smoke, and is thrown in the direction of the person for whom it is intended, to the accompaniment of certain words. It is the spirit of the object which goes out, enters the victim and makes him ill; the pains which are felt vary according to the type of thing blown.

A special use of *taling* to send serious sickness is that practised in conjunction with spirit stones. These work on the same lines as other spirit objects, but they are more powerful and only shamans possess them. Every shaman possesses a number for his own use. A spirit stone is a pebble or quartz crystal (fig. 1) with a strong spirit attached, such as *Imawali* the bush spirit, or some earth or tree spirit. It is this spirit which does the work desired. Stones are frequently sent to rival shamans and an interchange may occur—a spiritual duel or bombardment. They may also be sent to ordinary people.

Tobacco smoke is blown on a stone to provide the carrying force. It is thrown in the direction of the intended victim and whizzes through the air carrying the attached evil. A stone makes a special whistle as it travels and sometimes, during a shaman's séance, one can be heard approaching. The shaman, during his séance, intercepts the stone if it is conveying evil, perhaps using one of his own stones to capture it. In this way he protects himself and his patients. At the end of his séance he may show a small stone which, he claims, was blown to him. Sometimes a friendly shaman from another tribe may send a stone to an Akawaio shaman for use. Spirits also send these stones and a dead shaman teacher may send one to his living pupil. All these operations occur at a spiritual level and the stone is just the material manifestation of what is believed to be happening in the spirit world.

#### *Types of Blowing*

The uses of blowing are many and varied and even by the end of my research I had not obtained an exhaustive knowledge of them. All blowing can, however, be divided into two categories; it is either good or bad blowing. Types of good blowing seem to be more numerous than types of bad blowing, but this naturally arises from the fact that few people know, or would admit to knowing, anything much about the latter category. The following are some of the main types of good blowing which I encountered or about which I was told.

#### I. Good Blowing

(a) Blowing to bring good luck, etc.: Years ago people blew for successful hunting and fishing. A man took a leaf, any leaf would do, blew into it and said certain words before setting off on his hunting or fishing expedition. People also blew on their weapons in order to get plenty of meat and for this reason the owners of such charmed weapons would not sell them to others. The words for this type of blowing have now been forgotten.

A parent would blow on a son or daughter about to take a spouse, thereby conveying good luck.

Tobacco smoke is blown over food, gardens, plants and property in general, to make it grow or 'stay good' as the case may be.

(b) Blowing as a preventive measure: Blowing is frequently used to keep away rain. Several instances of this kind of blowing occurred at the beginning of the long wet season. As soon as a dark cloud threatened some old woman would dash out of her hut and blow vigorously in its direction and address it as follows: 'Go away, go away, go to another place, go away, go away' (*Mials, mials, turuna patabona uts, mials, mials*). Similarly, two Adventist Indians travelling in a canoe started blowing vigorously down their noses, muttering, 'go away, go away' (*mials, mials*).

An interesting use to which blowing may be put arose out of a visit by a Seventh Day Adventist mission Akawaio to a non-mission village. Among the various foods which Adventists are forbidden to eat is bush hog (peccary) and this Indian had claimed that he could not eat such meat

since it always gave him a headache. One day he was seen eating some. He had got a mature Indian to blow on him, he said, and could now eat hog without any unpleasant consequence to his health!

Blowing may be used as a protection from blood revenge and the attacks of others. Bagit, who had killed a man whom he believed to be *Edodo*, claimed that *taling* had kept away the man's son and protected him from blood revenge.

Blowing may help to keep away *Edodo*. When red paints, *saba* and *kurugai*,<sup>7</sup> are put on the face as a protection, to avoid seeing ghosts and so becoming ill, it is blown on first. This is not done to paint applied for purely decorative purposes.

Blowing is said to be effective in preventing pregnancy during the period of sexual licence frequently indulged in before a girl takes a husband.

(c) Blowing as a curative agency: In the majority of cases blowing is used to cure illness. Any type of illness benefits from blowing. I encountered the following:

1. Pimples and sores in the mouth of a baby of a mission family, resulting from dirty breast feeding.
2. For snake bite on the hand.
3. As a cure for fever (*walbaima*) a *kiali tabu*, or food stone<sup>8</sup> (Plate Db), is rubbed over the shoulders of the sick person to the accompaniment of gentle blowing. The spirit associated with the stone reinforces the blower's spirit and the virtue of his charmed words.
4. As a cure for *engup*, an infection of the eyes from an insect, causing swelling and inflammation of the lids, etc.
5. As a cure for an open sore at the neck, probably due to a tubercular gland.
6. For pains in the chest, stomach, limbs, etc.
7. One woman, who was about 35 years old, very much wished for children. She complained of pains at each menstruation which caused her to retire to her hammock and stop work. These pains, she believed, were due to someone who, at some time wishing to help her, had blown to give her a baby but had not managed it right. The result was, she maintained, that she got pains every month but no baby!

II. Bad Blowing. Bad blowing is designed to cause sickness and death. All sorts of aches and pains may be due to it and frequently to spirit objects sent by blowing. Tuberculosis of the lung was in two instances attributed to it. Whooping cough, fevers, sores, skin diseases and infections, either minor or serious, result from evil blowing; also blindness.

There appears to be no illness which is typical of bad blowing; any type may be sent by it. The fact that *taling* is the cause is diagnosed by spirit revelation to the shaman who passes on his findings to the patient and his relatives.

#### *Methods of Effecting Cures by Blowing*

Blowing kills slowly and sickness from it is believed to take a long time in being cured—if it is ever cured at all. If really effective bad blowing is sent it is extremely doubt-

ful whether a person can survive once it begins to take effect. However, whereas illness caused by *Edodo* attack can never be cured, that which derives from an evil *taling genak* usually can.

The only thing to do for a person sick from blowing is to get people to come and blow good on him and try by virtue of their words and the strength of their own spirits to drive away the evil forces which have been sent. Retaliation by blowing back may occur provided that the source of the trouble is known. Sometimes *taling* will not be suspected until after death, or until a shaman has diagnosed the exact cause of the trouble and whence it derives.

If the patient is really ill, the shaman is called in to provide spirit help. If a spirit stone or other object has been sent, the shaman—or rather, one of the spirits which assist him—intercepts it and retaliates in kind. The spirits come and blow good on the patient and the shaman may suck out and squeeze out any object which has been sent. I never encountered this latter type of cure, although there is frequent mention of it in the Guiana literature and Akawaio shamans maintain that they do this, though apparently not as a visible operation.

Akawaio maintain that the effects of bad *taling* are best cured by good *taling* but that all illness, whether caused by *taling* or not, may be cured by it. The one exception is illness caused by *Edodo* which is incurable, though even in this case *taling* is tried in the vague hope of a miraculous cure.

#### *Taling and Social Structure*

The reply to the question 'who practises bad *taling*' is frequently illuminating. Many will say that they do not know, or will generalize by making a reference to the 'next people' or, 'the people over there'—at the same time indicating various directions of the compass. In the more specific replies which are obtained, however, a certain pattern is apparent which is based on the tribal and inter-tribal structure, the cleavages and solidarity of the various groups existing in the society.

I. *Taling and River Groups.* It frequently occurred that the people of a settlement in one river group who were ill and who called in their shaman to diagnose the cause and obtain a cure, were found to be suffering from the effects of blowing coming from someone living in another river group.<sup>9</sup> Thus, Jane of the Mazaruni river group, who had several months' illness before succumbing to tuberculosis, was considered to be a victim of bad *taling*. The Mazaruni people thought that *taling* in this instance came from the Kukui people and that Jane was blown on during her visit to Amokokopai village shortly before the illness occurred. Similarly, the wife of Austin (Chinawieng, Mazaruni river group), was said to have died of *taling* coming from a man called Oli-oli, living at Amokokopai, Kukui River.

The Mazaruni people were firmly convinced that blowing came from the Kukui, the Ataro, or from the Kamarang; all but the people of Tagaikapai settlement, who had close relations in the Kako, considered that blowing might also come from the Kako river group.

At Amokokopai a girl, who had died of whooping cough, was said to have died of blowing sent from U'Wi village, Ataro river group. The Kukui people considered that *taling* might come from this or any of the other river groups.

In the Kamarang mission areas the general opinion of the origin of bad blowing was summed up by one Akawaio who advised me not to go up either the Mazaruni or its tributaries. 'The Mazaruni people are not too good,' he stated, 'they blow sickness on people.'

II. *Taling and Tribal Groups.* *Taling* may also come from the 'savannah peoples,' the Arecuna, Maionggong, Kamarakota, Potsawagok and other tribes.

In the case of Jane, whom I have already mentioned, a spirit séance was held which gave a final verdict—that the *taling* had come from the Arecuna on the Venezuelan Savannahs.

In a second instance, an Akawaio suffering from tuberculosis was sent to hospital in Georgetown but was discharged soon after admission because the disease was too far advanced. On his return the spirits revealed in a séance that he was a victim of bad blowing which came from a Macusi at the Amerindian depot in Georgetown.<sup>10</sup> He died shortly after.

'*Taling*,' Francis of Tagaikapai (Plate D, c and d) asserted, 'may come from anywhere, the Arecuna tribe, Kamarang and Kukui river groups, the Patamona tribe, even from the Mazaruni, for there are two bad people living at Imbamadai.'<sup>11</sup> As *taling* came from the Arecuna to Jane of the Mazaruni group of the Akawaio, so it might come from Germany to England, from a hostile group to another group, especially so in the past when the Akawaio and Arecuna fought as Germany and England have.<sup>12</sup>

The structural significance of *taling* accusations cannot be made clearer than by this example which Francis volunteered, and in all my enquiries concerning the origin of bad *taling*, and sickness and death arising from it, the structural factor was the predominant and determining feature. As the distance in the relationship increases and as the cleavages in the structure grow larger, from river group to neighbouring Carib-speaking tribe to foreign tribes, so the incidence of bad blowing increases. Good people blow good to you and these good people are one's friends, relatives and, in other than exceptional circumstances, your own river group as a whole. Bad people blow bad to you and these are your enemies, strangers, hostile people from hostile river groups and different tribes. It is noticeable that *taling* accusations and *Edodo* accusations follow exactly the same pattern.

#### *Taling in Personal Disputes*

Unlike *Edodo*, *taling* is regarded as a legitimate means of revenge for a wrong done. The victim will probably protest that it is evil and bad, but, generally speaking, it is a recognized mode of procedure. *Edodo*, everybody agrees, can never be good and is the enemy of all.

*Taling* sometimes plays a prominent part in Akawaio personal relationships, as the following incident shows:

Bagit of the Kukui river group fell ill and everyone

thought that this was due to bad blowing from the Kamarang river people. Then another cause was suggested. Some months before, Bagit had gone to Kataima to see Jacob there. He went to exchange a good 20-bore shot gun for an old one that required powder and shot but which was less expensive to maintain. Jacob's daughter died during his visit and it was thought that Jacob had sent *taling* in revenge for the death. Rumour and gossip from the Kataima people were responsible for this conclusion since they had linked Bagit's transactions over the gun with the girl's death. Bagit's brother, who, it was said, had wanted the gun which was bartered, threatened to challenge Jacob to personal combat<sup>13</sup> (cf. fig. 2) if his brother died of the fever. However, Bagit got better and the whole case soon became forgotten—or so it appeared.



FIG. 2. KATAIMA VILLAGE BOYS WRESTLING

*This is the traditional form of personal combat*

This case is typical of the part that blowing sometimes plays in personal relationships. Although in this instance the trouble arose between two people of separate river groups, the same type of dispute might occur between people of the same village, who are distantly related but on bad terms. Such instances are not frequent, for people do not blow bad on people of the same settlement except in the most extraordinary circumstances. Yet, wherever there are hostile relationships of any kind *taling* may arise from them and cause sickness or death, according to Akawaio notions of causation and recognized modes of behaviour in such situations.

#### *Taling and Other Societies*

Literary works on other Guiana tribes show that at least some of them have a blowing ritual like that of the Akawaio, but it is impossible to judge the extent of the similarity. The information given is too scanty except for the Barama

River Caribs, and no one has investigated these beliefs and actions as being part of a coherent system of thought.

Although a comparative study in the Amerindian field of investigation is impossible at present, the Akawaio blowing ritual is, on examination of its component parts, not by any means dissimilar from rituals and beliefs in other parts of the world.

As already stated, the theory of blowing postulates two important principles—the efficacy of charm words and the power of the spirit associated with the breath. The first component is clearly in the same category as magic or charm words used among widely differing peoples and found nearly all the world over. A good example is the type of magic spell described by Malinowski<sup>14</sup> in his account of the Trobriand Islanders.

Malinowski stated that magic power is an inherent property of certain words uttered with the performance of certain actions. The virtue of Trobriand spells lies in the words and in the voice. Voice and object are brought into the closest association whenever possible, and if the object to which the spell is directed is distant, the words are ejaculated in the appropriate direction. The effect which has to be achieved is such that demonstrable scientific proof is impossible to obtain. Unlike Akawaio blowing words, however, Trobriand magic is stored in the stomach; the words alone are efficacious and no type of spirit is associated with the spells. The performer's spirit is of no importance and the magic force issues in the words alone.

The second component in *taling* is the spirit of the blower which is associated with the breath. This association is not in itself unique for it is found in both primitive and complex societies and it, too, is practically universal. It has been reported among the Bantu people of Africa,<sup>15</sup> for example, and it exists in Christian beliefs.

The Greek *pneuma* and the Latin *spiritus* may both be translated as 'breath,' 'breeze,' 'the breath of life,' 'a breathing,' 'a gentle blowing of air.' In poetic usage and in the post-Augustan period *spiritus* was used in the sense of 'spirit,' 'soul,' 'mind.' This association of breath and spirit, which is probably a feature in all Indo-European languages, can be traced back to early Hebrew. In many instances, *ruach* has the meaning of 'breath' and, particularly, 'strong and violent breathing' as opposed to *neshamah*—which is ordinary, quiet breathing. *Ruach* also means 'life stuff,' 'source of life,' 'the life spirit.' It stands for Power and Life.

It might perhaps be objected that as *ruach* is of God and is given to man by God,<sup>16</sup> this association of breath and spirit in Christian doctrine is not comparable to the association of breath and spirit postulated in the Akawaio system of beliefs. It might be argued that *nephesh*, meaning 'breath-soul,' which is of man and not directly of God, is more appropriate.

The Akawaio do not recognize any distinction, so far as I could ascertain, between the soul and the spirit. The *akwahu*, the spirit of a person, comprises both together, being the 'sort of life' in a person<sup>17</sup>; it is his vitality and his individuality and yet it is also, by virtue of its implicit contact with *Akwa*, which is Light and Life in the sun's

place, something more than a thing of man. Therefore, the *akwahu*, in its association with the breath, is, as far as such a comparison is possible, like *nephesh* and *ruach* combined. As the semi-Christian Hallelujah<sup>18</sup> Akawaio today believe that God is in *Akwa*, in the sun's place, a person's spirit (*akwahu*) may be said to be 'of God' in the same way as the *ruach* in man comes from God in the Hebrew conception.

### Conclusion

In spite of comparable data from other societies it does not seem practical to make a detailed comparison between Akawaio ritual blowing and any other system of magic spells or religious beliefs outside the South American area. This is because of the great differences between the systems themselves and between the societies as wholes. However, the component parts in the theory of Akawaio blowing are not without parallel. *Taling* words may obviously be classified as a form of magic while the association of breath and spirit and its connexion with some superior religious force or power also occurs in other societies.

The unique feature in Akawaio beliefs lies in this combination. Also unique is the way in which the practice of blowing permeates all aspects of the society. It enters into the economic, legal, structural and religious spheres of society; it enters, in fact, into almost every department of thought and into nearly every social activity, so that to study it from one point of view, such as medical theory alone, is virtually impossible.

### Notes

<sup>1</sup> *Edodo* is the Akawaio term for 'Kanaima.' This word is used throughout British Guiana for 'a secret killer' who is said to catch his victim when he is alone, and to break his bones and poison him.

On recovering consciousness the victim returns to his village not knowing what has happened; he falls ill and dies within a short time. The fact that a person has been killed by *Edodo* is known by certain blue marks on the corpse.

<sup>2</sup> This is the consultation of the spirits of living people, ghost spirits and nature spirits, by the *Piaichang* at night.

<sup>3</sup> Chinawieng: a village on the Ayanganna plateau, Mazaruni River.

<sup>4</sup> Amokokopai: a village on the Kukui River, the religious centre.

<sup>5</sup> J. Gillin, 'The Barama River Caribs of British Guiana,' *Pop. Peabody Mus. Amer. Archol. and Ethnol.*, Vol. XIV, No. 2 (1936), p. 147.

<sup>6</sup> The verb *uro genang* = 'I have' or 'I possess.'

<sup>7</sup> *Saba* is a red pigment made from *Bixa orellana*; *kurugai* is made from *Arrabidae chica*.

<sup>8</sup> *Kialí tabu* is a special stone placed in the gardens, having associated with it the spirit of cassava and of gardens in general.

<sup>9</sup> All the people who live in the vicinity of a river, or a main tributary of a river, are known by its name.

<sup>10</sup> In Georgetown a building is set aside as a rest house for all Amerindians who arrive. This practice dates back to Dutch times when Amerindian assistance was sought against rebellious and runaway Negro slaves.

<sup>11</sup> This is a specific reference to a family which was inviting the opposition of the entire tribe, regardless of internal structural differentiation, through reckless boasting about *Edodo* activities.

<sup>12</sup> Francis had been in close association with a miner from the coastlands.

<sup>13</sup> This is the traditional wrestling match.

<sup>14</sup> B. Malinowski, *Argonauts of the Western Pacific* (1922), pp. 402-427.

<sup>15</sup> W. C. Willoughby, *The Soul of the Bantu* (1928), pp. 10, 64, 138, 181, 184.

<sup>16</sup> N. H. Snaith, *The Distinctive Ideas of the Old Testament*, Chap. VII.

<sup>17</sup> A. J. Butt, 'The Burning Fountain, from Whence it Came,' *Timehri*, No. 33, October, 1954.

<sup>18</sup> The present-day religion of the Mazaruni Akawaio is known as 'Hallelujah.' Hallelujah is a mixture of Akawaio beliefs and Christian teachings.

## SOCIAL FACTORS INFLUENCING INDUSTRIAL OUTPUT\*

by

TOM LUPTON

Department of Social Anthropology, University of Manchester

I  
**49** Far more than their British colleagues, American social scientists have been attracted to study output restriction by industrial operatives. In this article I shall examine the contribution which has been made by American research workers in this field and then refer to my own work in British industry. My work emphasizes certain interesting aspects of the problem which have so far been relatively neglected.

Research workers on industrial problems must return again and again to the investigations made at the Hawthorne works of the Western Electric Company in Chicago in 1927-1932. I shall refer particularly to the interpretations of the Bank Wiring Observation Room data. Although a

good deal of work has been done since, these interpretations have not been seriously challenged. There has been much general criticism of the Hawthorne work, but for the most part it has not been based on material collected as conscientiously, and as carefully analysed.<sup>1</sup> I shall take the Hawthorne interpretations as representing the general approach to the problem, although I shall refer to more recent work.

II

The operatives in the small workshop known as the Bank Wiring Room were observed and interviewed over a long period, and records of their output were carefully kept. The operatives were paid according to a group piece-work scheme. I need not describe this scheme in detail. Like most wage incentive schemes it rested on the assumption that the operatives would attempt to maximize

\* The substance of a communication to the Royal Anthropological Institute, 1 December, 1955

earnings up to the limit imposed by skill and physical fatigue. But, as Professor Homans has pointed out in his excellent re-analysis of the data:

'whether the men were expected to behave as described, the fact is that they did not. . . . They had a clear idea of a proper day's work. The actual output was not as great as it would have been if fatigue had been the only limiting factor.'

and

'Together with the belief that a man's output ought not to go above a certain limit . . . went the belief that a man's output record, that is, his average hourly output, ought to show little change from week to week.'<sup>3</sup>

The output graph approximated to a straight line. The output of the individual operatives was held to the group norm by a complicated process of social control operating within the work group.

In a recent British case of output restriction which led to a prolonged strike in the Scottish factories of the Rolls-Royce organization, an operative was ostracized by his fellows for 'rate busting,' that is, for producing more than they themselves considered to be a proper output. It was clear in this case, and in others which I have encountered in British industry, that operatives fixed a 'proper output' and attempted to apply sanctions to ensure conformity to it in the belief that they were protecting their long-run economic interests. They did this even though thereby they did not immediately earn as much as was possible.

The Bank Wiring Room investigators examined the 'long-run economic interest' hypothesis in the light of their data and rejected it. They argued that the actions of the operatives whom they observed were not based upon a logical appraisal of the work situation. They were able to show that the men did not understand fully the intricacies of the method of wage calculation. Without such knowledge it would clearly have been difficult for the men consciously to manipulate the situation to serve what they thought to be their own long-run advantage.

The investigators then went on to examine what may be called 'the quiet life hypothesis.' This states that operatives restrict and stabilize output at a certain level so as to ensure that the attention of the management will not constantly be directed to their activities: as, e.g., to see whether the incentive system is working efficiently, whether the layout of the shop can be improved, and the like. As against this the Hawthorne investigators argued that restriction of output may well lead to an increase in the unit cost of the product and this is the very thing which will call attention to the activities of the shop and bring about the very changes which the workers are assumed to fear.

Thus they rejected the argument that output restriction is the result of a shrewd calculation of certain crucial aspects of their situation on the part of a group of workers. Yet, when the operatives in the Bank Wiring Room were asked why they restricted output, these were the very reasons they gave. This apparent paradox can only be

resolved by calling the reasons 'rationalizations' and this is what the Hawthorne investigators did.<sup>3</sup>

Other possible interpretations were also considered and rejected. For example, the notion that workers are malicious, ungrateful, or just plain lazy was thrown out for lack of evidence from the Bank Wiring Room. The suggestion that the behaviour of the operatives was a manifestation of overt management-worker hostility was rejected for the same reason, and was replaced by the interesting suggestion that although there was in fact some hostility towards the management, this could be explained as emerging from the workers' endeavours to maintain the internal organization of their group. Worker-management hostility could not be advanced as a reason for restricting output.<sup>3</sup>

I am on fairly safe ground when I say that the studies of operative groups since Hawthorne have for the most part adopted similar interpretations.<sup>4</sup>

### III

What worries me about these interpretations is their too easy assumption that the fears of operatives about expected management behaviour are unfounded. Once this position has been taken up, field material on output restriction can only make sense if it is stated that the fears are irrational and emerge as a sort of by-product of the social process within the work group. I do not deny that this is a valid interpretation of the Bank Wiring Room data. But I do not think it will do very well for the British cases I have briefly mentioned, nor does it help me with my own material, as I shall show presently.

I want to direct special attention to the argument which I believe underpins the 'Human Relations' approach to industrial problems, and which is implicit in the Hawthorne interpretations. The argument is, that because it can be shown that in a particular case an employer deals fairly with, and is solicitous for the welfare of his employees, any restriction of output must be traced to causes other than conflict of interest between them. I do not think that this argument is sound. I would rather say that the worker-employer relationship is one in which both conflict and identity of interest are present at the same time. The balance between conflict and identity may of course be modified by circumstance. I do not admit, however, that the element of conflict of interest can be entirely removed from the relationship either by the adoption of enlightened personnel policies or paternal attitudes. I would say that these can shape the procedures which are followed in handling conflict of interest. I take up this general position because to me it is evident that though workers and employers share a common interest in continued cooperation, they cooperate for different ends. The employer and his managers are interested in the production and profitable sale of a commodity. The worker is less interested in this than in the material and other satisfactions which he can get from his employment. Everyone is aware that these ends are often in conflict. This being so, there may often be good grounds for workers' fears. The possibility must then be admitted that restriction of output might in some cases be explained straightforwardly in terms of these fears. It

may not in all cases be necessary to follow the tortuous line of reasoning that leads to the conclusion that fears of rate-cutting and interference are irrational.

I have said enough to have made clear the problem in which I am interested and I have discussed possible ways in which data on output restriction may be interpreted. I have not the space to enter into great detail about my own research, but I shall say something about the conclusions which are emerging from the analysis of the material. I will then relate these to the interpretations which I have just been discussing.

## IV

I entered a factory as an operative. After a short period of training I joined a work team. I remained with this small group for an extended period. I worked, watched, and listened. Everyone with whom I associated knew that I was a University man. Most knew vaguely, and some precisely, the object of my presence there. The management were kind enough to allow me access to the output records of those workers whom I knew and observed. Before, during, and after my spell as an operative I discussed my research problem with managers and with Trade Union officials. I have also obtained a good deal of material on the history and present-day structure of the industry.

From all that I had read, and from my own industrial experience, I had expected to find evidence of some kind of production norm (*i.e.*, of a standard rate of production established by a group of workers, differing from management expectations). I had expected to study the process of social control by which the norm was maintained in the workshop. The notes I made recording my impressions of the first few days in the shop seem to show slight bewilderment at finding that nothing I had ever seen or read about seemed to be present in this situation. Subsequent analysis of the output records confirmed this.

Output in the Bank Wiring Room followed a straight line. Other investigators have reported situations where a norm of appropriate daily earnings was maintained,<sup>5</sup> or where output followed a bi-modal pattern.<sup>6</sup> In the shop where I worked I found nothing like this. The output graph for the group which I studied showed marked fluctuations. These fluctuations have to be explained not by reference to the internal social organization of the group but by reference to factors over which the operatives have no control, *e.g.*, changes in the design of the product, disturbances in the flow of work through the factory, difficult materials, and the like.

The graphs of individual output over the period also exhibited marked fluctuations. But these fluctuations in individual output did not correspond with each other in the majority of cases, nor with the fluctuations in the group's output except in the case of those operatives engaged upon the final assembly of the product. Since the operatives were on straight piecework it follows that earnings fluctuated with output. The extent of the output fluctuations, as reflected in earnings, was well illustrated by the following figures. The maximum wage of the opera-

tive whose output graph shows the least marked fluctuations was roughly 10/- above her own average for the period, and her minimum wage about 10/- below average. The operative whose graph showed the most marked fluctuations had a maximum wage £1 7s. od. above her own average and a minimum wage £2 10s. od. below. These figures are calculated on all full weeks worked.

There were social controls operating in the group, but these were dictated not so much by any social norm fixing a 'proper output,' but by the necessity for the operatives to see that they were kept supplied with work by those preceding them in the production flow.

The unmistakable facts of the situation, for all my initial attempts to fly in the face of them, are that no norm of 'proper output' has emerged from the internal organization of this group of workers. My findings differ from those of the Hawthorne investigators and of others who have studied the problem. I have already described how they interpreted the facts they observed. My task is to interpret the facts of the situation I myself observed and experienced.

I cannot say that the fear of rate-cutting was entirely absent. Some workers held the view, and expressed it to me, that the time-study men never do a re-time to raise a rate. But although there was some fear of rate-cutting I never saw a rate cut while I was in the shop. Many people complained that certain rates were too 'tight,' but I saw and heard of cases where workers asked for a re-time and managed to get rates raised. Although there was some dissatisfaction and some fear of rate-cutting no *collective* restriction of output was practised. In fact such group pressures as operated to affect individual output were mostly the other way. I myself was often jockeyed gently along by operatives who were short of parts for sub-assembly.

At first, I considered the following hypothesis. An output norm has often been regarded as a factor associated with group cohesion. Would not the absence of a norm indicate lack of cohesion? If this hypothesis were to stand I had to produce evidence of lack of group cohesion. I would expect to find this, if it existed, in the labour turnover, absence and sickness figures, and in a chilly and unfriendly atmosphere in the group. I found some evidence that the absence rates were somewhat higher than would normally be expected for a group of this kind, but labour turnover was surprisingly small. The social atmosphere in the shop was the reverse of chilly and unfriendly.

I asked myself whether the joking which characterized many relationships was a cover for personal hostility, a means to overcome awkwardness in these relationships. It is difficult to decide about this, but I think that the type of joking relationship I saw and participated in probably had more to do with friendship and familiarity. Many of the workers had known each other well for many years. They came from the same congested working-class area, and generally they shared the same set of values, tastes and beliefs. Certainly there was no awkwardness of the kind arising from differing social position, which gives rise to the joking relationship as described by Radcliffe-Browne.<sup>7</sup>

I fear that, attractive though it was, I have had to drop the *anomie* hypothesis. It does not accord very closely with the facts.

I have also tried to interpret the facts in terms of unquestioned acceptance of management norms and expectations, and the success of management controls over operative behaviour through the productive system, the wage incentive scheme, and effective supervision.

Like most managements, the management of the factory at which I worked hoped that the economic incentives they offered would make for high production. They were largely justified in their expectations. Most workers sought to maximize output, although practice differed from operative to operative according to economic need.

In this shop, the effect of layout on clique formation, a point which has been stressed by some investigators, was not marked. The pattern of group formation at break and meal times was persistent but it was based on age and upon long friendships which had survived different layouts of the plant. Indeed, the layout of the shop and the working arrangements tended in most cases to limit the development of informal social organization based on spatial propinquity.

Reference to previous work suggests that the absence of such informal organization would inhibit the development of social control over output in the shop. In the Bank Wiring Room there was a close connexion between three sets of factors, spatial propinquity on the process, clique formation and social control of output; and other investigators have noted this connexion.<sup>8</sup> In the shop where I worked the connexion is not marked. But even given that the shop layout, etc., tended to inhibit it, there was scope for the social control of output within the group far beyond the very little that existed. What still has to be explained is why in this case it did not emerge.

This can only be explained if the analysis is extended to include material other than that which I collected within the workshop. The workshop has now to be considered as part of a system of relationships embracing the factory itself, the industry, the Union, and the wider society of which they are all a part.

The industry is highly competitive, and the market for its product is subject to marked seasonal and longer term fluctuations. It is not surprising then that managers in the industry are acutely cost conscious. And to be cost conscious in an industry like this, where labour cost forms a high proportion of total cost, is to be particularly sensitive about labour cost. There are two chief ways of keeping labour cost down; keep wages down and/or use labour more effectively. In this industry, in this time of full employment, serious attempts at wage reduction are not attempted, and since the industry is one which does not lend itself easily to mechanization many firms have introduced schemes involving changes of layout and job breakdown to permit the most effective use of the existing labour force and the employment of semi-skilled labour.

The firm in which I worked has, in recent years, reorganized many of its workshops. It has used the most up-to-date techniques to eliminate unnecessary movement

of people and materials in the productive process, and to standardize job method. Rates are now based upon time study and job evaluation. In some firms the introduction of such techniques might have given rise to collective resentment. In this case the operatives have accepted the techniques. Some operatives spoke to me nostalgically of the old methods, but by and large the new ones have been accepted without much fuss. I think that conflict on this issue has been minimal because the management and the operatives have this in common: they want to do as well as they can while the orders are coming in. Pieceworkers with memories of seasonal unemployment and short-time working work hard to make hay while the sun shines, and although for the past few years the fluctuations in demand have been much less marked than was the case in the pre-war years, the habit still persists.

There is no tradition in the firm of workshop bargaining over piecework rates. The Trade Union safeguards the minimum rates upon which the piece prices are based, but it is not party to the disputes which arise in the workshops over individual rates, nor do its shop stewards participate in the rate-fixing process. In the Rolls-Royce case which I have mentioned, and in other cases which I have met, workers who discipline 'rate-busters' are skilled in the ways of using the power which Trade Union membership confers in times of full employment. In the shop where I worked there was no evidence of skills of this kind. Although the Union which organizes these workers has grown in influence over the past few years and has minimum wage and price-list agreements with most firms, its influence is not marked in the daily give-and-take of workshop life. At least this was my experience in the shop where I worked.

This does not affect my argument that conflict as well as identity of interest is typical of the management-employee relationship. I saw many cases of conflict of interest but I noted that in this shop they were handled individually and not collectively. The development of a 'look after Number One' attitude of mind seems to be due largely to the fact that individual piecework and seasonal employment are characteristic of the industry. The other reason which may be advanced to explain this attitude is that most of the operatives of whom I have spoken are women. It was true of women in this shop that they tended to accept management norms of right behaviour and that their attitudes to output conformed fairly closely to management expectations. Where conflict did occur over such matters as rates and job transfers these were handled by the individuals concerned. There is evidence from other studies that women prefer individual to group payment.<sup>9</sup>

I am not convinced that the sex of operatives is an adequate explanation of attitudes to the job and to management. My information about male operatives in this industry is that they behave similarly. And both men and women in the industry have been known to act collectively when general standards have been threatened. But, as I have said, collective action in the workshop does not appear. I would say that this can only develop when the Trade Union has become more powerful there.

The women operatives I worked amongst stood in awe of management—the 'higher ups' or the 'brains,' as they were sometimes described to me. But this is not an adequate explanation of their behaviour in relation to output. In my experience many male workers have the same attitude and this attitude exists side by side with restrictive output norms in some industries. The recognition that management has power and know-how is not necessarily associated with the acceptance of management norms and expectations.

It would seem that in the case I have been discussing the influence of controls operating upon the group from outside has a greater effect on output than those which operate within the group. This suggests to me that it is only when there exists a well developed system of workshop collective bargaining, based upon strong Trade Unions, that the kind of workshop solidarity which allows of operative control over output can emerge. Of course this solidarity is built upon and emerges from social relationships within the workshop; but it is shaped also by the structure of the management-worker relationship. This in its turn is shaped by the competitive structure of the industry, and the extent and power of Trade Union organization, and the attitudes which management and workers bring to the situation from their experience of membership of social groupings outside the factory situation. My feeling is that worker-management relationships, as a group of workers perceives them, are shaped not so much by mythical or irrational expectations of management behaviour as by traditional attitudes built into the relationship.

## v

My opinion, based on the Hawthorne interpretations, on the analysis of my own material, and on my fairly wide knowledge of a number of British industries, is that the study of production norms is incomplete unless it takes account not only of the minutiae of workshop life but also of the whole complex of traditional and structural factors which help to make these relationships what they are.

This article has been full of hypotheses examined and rejected. I shall conclude by stating one which seems to accord with the known facts.<sup>10</sup> But first I will refer to the fact that the American investigators of the production norms problem make very little mention of Trade Unions. This is probably because the role of the Unions in the two countries differs. And, of course, the two Trade Union movements have followed different paths of historical development. It is not to be taken then that the stress that I place upon the role of Trade Unions in the life of the workshop is intended as a criticism of the work of American colleagues. It is not. I have the greatest admiration for their work. Without it I would not have known where to have begun in my own studies. The hypothesis that I state relates to British conditions.

I would say that restriction of output is more likely to be found in workshops in industries where Trade Unions are

strong and where they are also well organized at workshop level. The other factors which might be associated with restriction of output are a high capital-labour ratio, which relieves downward pressure on wage rates, manning scales and the like; and an absence of keen competition between the firms which make up the industry. . . . This complex is, so to speak, woven around the relationship between management and work group, giving a kind of security which allows of collective action by the work group to control the situation in which it finds itself. The way in which control is exercised, and its final result, will be shaped by factors specific to each situation: the degree to which the process is machine paced, the layout of the plant and so on.

The complex of factors which I would expect to be associated with lack of social control over output in the workshop would include lack of monopoly and cartelization of the industry, lack of intensive capitalization, lack of strong Trade Union organization inside the workshops, and instability in the market for the product. In this situation the investigator's problem is to explain how, in the absence of control over output, stability is maintained in the social relationships of the workshop; what other norms of behaviour emerge and what is the pattern of sanctions associated with them. I have not examined this problem in detail in this article although I have touched upon it.

## Notes

<sup>1</sup> Cf. W. A. Koivisto, 'Value, Fact and Theory in Industrial Sociology,' *Amer. J. Sociol.*, Vol. LVIII (May 1953); Harold L. Sheppard, 'Approaches to Conflict in American Industrial Sociology,' *Brit. J. Sociol.*, Vol. V, part 4 (1954).

<sup>2</sup> G. C. Homans, *The Human Group*. New York (Harcourt, Brace & Co.), 1950, pp. 60-61.

<sup>3</sup> F. J. Roethlisberger and W. J. Dixon, *Management and the Worker*. Harvard U. P., 1939.

<sup>4</sup> Cf. O. Collins, M. Dalton, and D. Roy, 'Restriction of Output and Social Cleavage in Industry,' *Applied Anthropol.*, Vol. V, part 3 (Summer, 1946). These investigators write:

'Analysis has indicated that restriction of output is an expression of group solidarity, arising from causes other than economic fear, laziness or dishonesty,' although it 'at the same time expresses a feeling that workers are different from management, that their motivation and goals are quite apart from those of management oriented employees.'

<sup>5</sup> S. Robbins, *The Human Factors in Industrial Output*. (Unpublished Thesis for degree of A.B. (Hons.), Harvard College.)

<sup>6</sup> Donald Roy, 'Quota Restriction and Goldbricking in a Machine Shop,' *Amer. J. Sociol.*, Vol. LVII (1951-52).

<sup>7</sup> A. R. Radcliffe-Brown, *Structure and Function in Primitive Society*, London (Cohen and West), 1952.

<sup>8</sup> Cf. A. B. Horsfall and C. M. Arensberg, 'Teamwork and Productivity in a Shoe Factory,' *Human Organization*, Vol. VIII, part 1 (Winter, 1949).

<sup>9</sup> J. F. Scott and R. Lynton, in their study of change in a hosiery factory, describe how an 'attempt to introduce group payment was abandoned at the workers' request.' *Three Studies in Management*. London (Routledge & Kegan Paul), 1952, p. 125.

<sup>10</sup> S. Wyatt's 'Study of Output in Two Similar Factories' is a possible exception. *Brit. J. Psychol.*, Vol. XVIV (1952). I am grateful to Mr. R. G. Stansfield, of the Department of Scientific and Industrial Research, for drawing my attention to this.

# ROYAL ANTHROPOLOGICAL INSTITUTE PROCEEDINGS

**Land Rights in Two Bremnes Hamlets.** By J. A. Barnes,  
D.S.C., M.A., Ph.D., London School of Economics and  
Political Science. Summary of a communication to the Institute,  
15 March, 1956

The process whereby rights in land are transferred from one generation to the next is influenced not only by rules of inheritance but also by technological conditions, social and spatial mobility, and the accidents of fertility whereby one man may have many children while his neighbour has none. Changes in technology, social mobility and social structure may be accompanied by changes in the way in which property passes down the generations, despite unaltered inheritance rules. This proposition is tested by an examination of the transfers of land rights that occurred during the period 1851-1951 in two hamlets located in the rural district of Bremnes, in western Norway.

Under Norwegian law, all children, male and female, inherit equally. However, the eldest son has the right to buy land from his father at a reduced price, and all a man's descendants, in a fixed order of priority, have the right to buy back patrimonial farm land if it has been sold to a stranger or to a more distant kinsman. Thus in effect the progressive fragmentation of holdings is hindered by the opportunity given to the eldest son to buy the shares, actual or potential, of his younger brothers and sisters. The law discriminates against incoming strangers in favour of kin, and against distant kin in favour of close relatives, provided they have the money. The interests of near kin are further protected by legal restrictions on testamentation. When a farmer wishes to retire from active work, he often sells his farm to a son. When the old man dies, his possessions, including any proceeds remaining from the sale of the farm, are divided equally among his children.

A hundred years ago, all the registered units of land in the two hamlets were farm holdings of the same broad category. Each consisted of a known fraction of the hamlet as a whole, and provided a means of livelihood for a simple family with a few adherents. Holdings were not consolidated and all land was owned jointly, although the hayfields and the small tilled patches were cultivated by the households severally. Most adult men combined agriculture with in-shore fishing, and some kinds of fishing were carried on by the hamlet as a unit.

In the latter part of the nineteenth century the compact cluster of houses which hitherto had formed the nucleus of each hamlet was dispersed, and at least the in-field of each hamlet was divided into a number of compact blocks, one for each holding. At the

beginning of the twentieth century, larger fishing vessels came into use, and it became possible, and sometimes advantageous, for a man to live by fishing alone. Later, industries became established in the district, providing full-time employment for men from many hamlets. Fishermen and industrial workers became sufficiently prosperous to buy land for houses from their farmer neighbours.

During the same period the old taverns catering mainly for the coastal sea traffic began to be eclipsed by the country stores which, although built on the quayside, aimed at providing a wider range of goods for residents in nearby hamlets. More recently, several shops with a narrow range of goods for sale have been set up in the centre of the district, serving a wider market than the country stores but quite divorced from sea traffic. Prayerhouses and schools were built in several hamlets at the beginning of the century, but the later development of secondary and technical education has been based on two schools serving several rural districts.

Considerations of kinship did not influence the acquisition of land for schools and prayerhouses, and influenced only slightly the sales of shop sites. Some influence can be seen in the sales of house sites.

All the farm buildings extant in 1851 in the two hamlets were divided to make more farms during the following hundred years. Draining and clearing of former pastures enable the new units to continue to provide a living for a family, although most men spend some time fishing during the winter. In two-thirds of the instances examined, the farm was divided to provide a holding for a younger son or for a daughter.

In a sample of 40 farmers who had farms to dispose of, one third sold the whole farm to their eldest son, another third sold their eldest son only a part, and the last third sold nothing to the eldest son. Those eldest sons who did not acquire any patrimonial land either went abroad, or to town, or followed other occupations locally. Only one of them bought land from someone else and became a farmer; 17 other farmers had no sons, and were forced therefore to sell to other relatives or to strangers.

Part of the apparent deviation from the pattern of primogeniture is thus seen to have been due to the accidents of fertility, part has been due to the provision of certain common services in the hamlet and to the more intensive exploitation of farm land, and part has been due to the growth of opportunities for non-agricultural occupation in the district and to immigration to other areas.

## SHORTER NOTES

**A Technique for Tabulating the Kinship Structure of Households.** By Michael Banton, Social Sciences Research Centre, Edinburgh. With four tables

51 There is a growing interest in the elaboration of quantitative fieldwork techniques among social anthropologists. Other research workers may therefore like to know of a means of coding and tabulating data on the kinship structure of households which I used in connexion with a sample social survey undertaken in Freetown, Sierra Leone, in 1953.

All data relating to each household was noted on a single filing card. The relationship of each individual member to the household head was represented by a code number of two digits, the

first denoting his or her generation and the second his or her degree of relationship to the head (see Table I). Thus the household head was always represented by the number 30. The code number of a person in the first ascending generation began with a 4, and in the second ascending generation with a 5. The code numbers of persons in descending generations began with 2, 1 or 0. Kinship relationships were coded according to the canonical reckoning, with minor modifications. Within the first degree were included the household head's parents, siblings and children; the code number of such persons ended in 1. Within the second degree were reckoned grandparents, parents' siblings, siblings' children, first cousins and grandchildren; all such persons were

given a code number ending in 2, but any danger of confusion between grandparents and, say, grandchildren, was obviated by the first digit denoting generation. The marriage tie was counted as an additional degree of removal, thus siblings' spouses were reckoned in the second degree, as were half-siblings and step-children. In this way the household head's brother was coded as

TABLE I. CODE

Generation	Household Head	Degree of Relationship to Head				Degree of Relationship to Wife			
		1st	2nd	3rd	4th	1st	2nd	3rd	4th
Second ascending		52	53	54		57	58	59	
First ascending		41	42	43	44	46	47	48	49
Head's generation	30	31	32	33	34	35	36	37	38
First descending		21	22	23	24	26	27	28	29
Second descending		12	13	14		17	18	19	
Third descending		03	04			08	09		

31, his grandmother as 41, his brother's child as 22 and his grandchild as 12. The household head's wife was represented by the code number 35 and those of her kin staying in the household had their positions calculated by reference to her own. Thus the wife's mother was coded as 46 and the wife's half brother's grandchild as 29. Boarders, servants, wards and visitors were given a special code letter. This method of coding is obviously rather crude, but it should be easy to refine it when a relatively homogeneous population is to be studied, while the use of such a device makes quantification very much easier.

All individuals having been classified in this way, the occurrence of the various relationships in households with a male head could then be expressed as in Table II. Where households had a female

TABLE II. COMPOSITION OF HOUSEHOLDS WITH A MALE HEAD

In 211 households\* the number of persons occupying given positions in the kinship structure was as follows:

Generation	Household Heads	Degree of Relationship to Head				Degree of Relationship to Wife	
		1st	2nd	3rd	4th	1st	2nd
First ascending		10	0	2	0	1	0
Head's generation	211	23	11	1	0	138	5
First descending		220	21	2	0	3	2
Second descending		17	3	1		0	0

Persons not related to Household Head:

Boarders: 22 Servants: 13 Wards: 40 Visitors: 17

\* 41 of the above households consisted of a single male.

head an extra code letter was used and these cards were sorted separately. One advantage of this method of tabulation is that it enables the reader to get an ocular picture of the data which corresponds closely to the significance of the figures.

Tables of this kind are primarily of use for making comparisons between, say, different tribal groups within an urban population

or urban and rural households among the same tribe. The decrease in household size concomitant upon urbanization can be demonstrated, as in Table III, by arranging the numbers of households of different sizes in the two populations as frequency

TABLE III. SIZE OF HOUSEHOLDS IN TWO TEMNE VILLAGES AND IN FREETOWN

These data relate to 44 households in two villages in Port Loko district surveyed by Dr. Littlejohn in 1954, and the 43 Temne households which formed part of the author's social survey of Freetown.

Number of Persons in Household	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15-19	20
Villages	...	2	4	4	5	4	3	5	1	2	1	1	5	3		
of households	Freetown	12	3	7	8	2	5	3	1	1	1	1	1	1	1	1

distributions. But though this shows the shrinkage in household size it would be difficult to indicate by these means how the decline in size is related to kinship structure. This can be done by constructing and setting side by side two tables similar to Table II discussed above. Alternatively, the two sets of figures may be combined, as in Table IV. If the two populations are of different size and each is sufficiently large, they may be adjusted to a common base number. Table IV shows where the shrinkage indicated

TABLE IV. COMPARISON OF KINSHIP STRUCTURE OF TEMNE HOUSEHOLDS\* WITH MALE HEADS IN TWO PROTECTORATE VILLAGES AND IN FREETOWN

In 41 village households and 34 Freetown households the number of persons occupying given positions in the kinship structure was as follows (the first of each pair of figures relating to the village households and the second to those in Freetown):

Generation	Household Heads	Degree of Relationship to Head				Degree of Relationship to Wife	
		4th				4th	1st
		1st	2nd	3rd	& 5th		
First ascending		9	2	3	0	0	0
Head's generation	41	34	24	6	14	1	2
First descending		93	38	49	2	6	2
Second descending		1	1	1	0	6	0

Persons not related to Household Head:

Boarders: (v) 28, (f) 6 Boarders' dependants: (v) 12, (f) 0  
Wards: (v) 2, (f) 2

\* Eight of the Freetown households consisted of a single male.

in Table III has taken place. In Freetown the number of elementary family relationships per Temne household is appreciably smaller than in the villages; the number of wives falls by two-sevenths and of children by a half (this may be due in part to the number of relatively young immigrants in Freetown). However, the decrease in extended kin relationships is very much more pronounced—witness the decline in the number of heads' siblings' children to one-twentieth of that in the villages. Differences between two sets of data of this kind can be accurately described only by tabulation.

There are certain obvious limitations to this technique. It indicates quite clearly the spread of kinship ties within the households of selected groups but gives no indication of the existence

of different types of household structure within those groups. Apart from the frequency of matricentric households this problem did not appear important in the Freetown survey. But it should not prove difficult to deal with along similar lines. An accepted technique for coding is perhaps the key problem as this would do more than anything else to enable field workers to present their data on this question in a way that will facilitate comparisons.

**Hæmoglobin C in Berbers.** By C. A. Pasternak, Department of Biochemistry, and D. F. Roberts, Anthropology Laboratory, Department of Human Anatomy, University of Oxford

52

Blood from a small sample of Berbers was examined for abnormal hæmoglobins. The 48 subjects were drawn from a number of tribes living in the High Atlas, Anti-Atlas and Saharan foothill zones of French Morocco, in the region of Ouarzazate, where the examination took place. The subjects were all male and unrelated to each other.

No sickling was found in any of the individuals tested. In paper electrophoresis, using a barbitone buffer solution of pH 8.8, the hæmoglobin specimens from 46 of the subjects migrated as a single band, with the same mobility as control specimens of normal adult hæmoglobin (haemoglobin A). In the specimens from each of the remaining two subjects, however, two com-

ponents were clearly distinguishable. The first component was hæmoglobin A, the rate of migration being identical to that of the above specimens. The second component was of lower mobility than control specimens of hæmoglobins A and S; that it was hæmoglobin C was indicated by comparison with known C specimens. The two subjects possessing this abnormal hæmoglobin were of different sub-tribes and birthplaces.

These results suggest that further investigation of the incidence of abnormal hæmoglobins in the Berbers of this region would be profitable; especially interesting would be a comparison with the more remote Atlas tribes. Hæmoglobin C has also been reported from Algeria.<sup>1</sup> It seems likely that the trait has been transmitted to North Africa across the Sahara from the areas of its greatest frequency in West Africa,<sup>2</sup> an interpretation with which some Rh blood-group evidence<sup>3</sup> is compatible.

Notes

<sup>1</sup> A. R. Portier, R. Cabannes, J. Massonat and J. Duval, *Algérie Médicale*, Vol. LVIII (1954), p. 557.

<sup>2</sup> G. M. Edington and H. Lehmann, *Trans. R. Soc. Trop. Med. Hyg.*, Vol. XLVIII (1954), p. 332; see also H. Lehmann and J. P. Mackey, *MAN*, 1955, 200.

<sup>3</sup> A. E. Mourant, *The Distribution of the Human Blood Groups* (1954), p. 78.

## REVIEWS

### GENERAL

**Motif-Index of Folk Literature, Vol. I.** By Stith Thompson. New enlarged and revised edition. Copenhagen (Rosenkilde & Bagger), 1955. Pp. 554. Price 92.50 kroner

53

Professor Thompson promises in a few years' time the completion of the Motif-Index, which will reach twice the size of the first edition published twenty years ago. In view of the surprising number of recent representative folktale collections, covering many parts of the world, the impressive speed with which material newly made available has been incorporated, speaks highly for the efficiency of the original systematical classification. Minor alterations will even further facilitate the inclusion of future additions.

The admirably produced new edition represents the truly monumental work of a lifetime and of successful teamwork. The entries depend, of course, upon the quality of the books and papers from which they have been excerpted. Nobody could expect the compiler to have made a special study of any item listed in this comparison of single 'narrative-motifs of peoples in all parts of the world and in all types of social and industrial development,' as the author announces in his paper *Narrative Motif-Analysis as a Folklore Method*.

I feel only entitled to judge the impact of this new edition in regard to Celtic folklore. The specialist will continue to consult the fairly up-to-date *Motif-Index of Early Irish Literature*, which was primarily intended for the use of students. In its preface T. P. Cross admitted that, in time and place, the range of his work is much wider than indicated by the title. It can be foreseen that the folklorist and student of comparative literature, not to mention the general reader, who will in future consult only Professor Thompson's reference work, will often be misled and attribute items to early Irish literature, which belong, as a matter of fact, to either mediæval romance or modern folklore, and in some cases to early Welsh folklore. This regrettable defect will increase the pitfalls besetting the path of folktale research. Other fields of work have presumably fared better.

By now the classification of narratives has so far progressed as to suggest another gigantic task of similar importance: a motif-index of art. In the past folktales have been recorded haphazardly, and though the number of ancient traditions which they have preserved is truly astounding, much that has faded from the memory of storytellers can still be gleaned from actual 'monuments of folklore.' A motif-index of Celtic art, for instance, would considerably enlarge

some sub-groups which are at present so small that they suggest to the uninitiated a subject of minor importance. One example should suffice to bear out my statement: god with hammer (A.137.1). A motif-index of High Crosses should provide much evidence of pagan and early Christian legends.

E. ETTLINGER

**Studies in Ancient Technology, Vol. I.** By R. J. Forbes. Leiden (Brill), 1955. Pp. ix, 194. Price 16.50 guilders

54

Over half of this book consists of a second edition of *Bitumen and Petroleum in Antiquity*, a standard work which well deserves reissue. Philology, geology and chemical analysis are all brought into service in the study of the sources, treatment and use of these substances in early times. Particular care is taken to decide the antiquity of the crucial art of distilling; could one also regard as evidence the story, recounted by Gibbon in his Chapter XLIX, of the 'water' which, when sprinkled over the veronica of Edessa, added fuel to the flames of the Persian besiegers in the sixth century A.D.?

The other two essays deal with wider topics, and are much less exhaustive. That on the origin of alchemy reveals the wealth of experiment in early Egypt and Mesopotamia. But the exact distinction between the suggested later stages in the development of the subject is not always clear, and only a few hints are given concerning the probable links between Near and Far Eastern alchemy. The dissertation on water supply gives a very complete study of Roman techniques, but a less full treatment of early canals, siphons and tunnels, and no account of the mechanical devices for raising water which were invented in the Hellenistic world and are now important in many parts of Asia.

But even if they are selective, historical essays of this sort are so rare that they are of great value, especially when supported, as in these instances, by copious bibliographies. It is, however, a pity that the reader's confidence in the book is weakened by the many simple mistakes of idiom and spelling.

W. C. BRICE

**A History of Mechanical Inventions.** By A. P. Usher. Revised Edition, Harvard (U.P.); London (Cumberlege), 1954. Pp. xi, 450. Price £3.12s.

55

Mr. Usher has added to this revised edition of his book four chapters setting out a general theory of the way in which mechanical inventions arise. It is difficult to follow the argument,

which is expressed in esoteric language, but the main thesis seems to be the unexceptionable one that great inventions follow from the gradual accumulation of slight improvements. A useful distinction, however, is made between the empirical kind of mechanical evolution and what might be called theoretical advance. In this second case, following the work of a man of genius, or some major discovery, like the application of the spring to the clock, the principle of the machine and its potentialities suddenly become clear.

The title of the book is misleading, for it is not a complete history but rather a collection of essays, dealing especially with sources of power and methods of keeping time, and also with textile and printing apparatus, and the achievements of Leonardo da Vinci. The descriptions and illustrations are admirably clear, precise and detailed,

and supported by abundant references. The analysis of each subject is, however, rather narrowly chronological, and questions concerning the transfer of ideas from place to place are either not asked or inadequately answered. Was the Chinese invention of movable type, for instance, certainly independent of that in Europe, and what is the exact evidence for the early working of wild silk in Asia Minor? Again, was there any connexion between the windmills of China and those of Europe? The evidence from Seistan is crucial here, and this is fuller than is implied on p. 172.

The book is carefully finished, though *aberration* is misspelt on p. 42 and *Harrison* on p. 327, and contrary to what is implied on p. 308, the equation of time changes only with time and not with position.

W. C. BRICE

## AMERICA

**Culture and Experience.** By A. Irving Hallowell. Philadelphia (U. of Penn. P.), 1955. Pp. xvi, 434. Price £2 15s.

**56** This is a collection of Professor Hallowell's papers which have appeared previously in various journals. He has edited the material himself and has chosen from his papers only those with direct bearing on the theme of the book. In order to elucidate that theme, five selections which have not been published elsewhere have been added. The resulting volume thus forms a more unified effort than is usual in collections of articles.

The theme is suggested in the title and is felt by Hallowell to be a central problem towards which many anthropological data converge. In his own words, it concerns the 'unique qualitative aspects of human existence that arise out of conditions of human experience which are not simple functions of man's organic status alone, and that have variable as well as constant features' (p. viii). This is not thought by him to be a new concern in anthropology. But it is one on which modern techniques in psychology may be brought to bear. The problem may be said to be basic to that old definition of anthropology as 'the study of man' which is now sometimes smiled at by sociologists. It concerns the human-ness of human existence, and more perhaps than any other anthropologist Hallowell has narrowed the geographical range of his data in order to devote his energies to the theoretical contributions made to this theme by a combination of anthropological and psychological viewpoints. In his control of psychological concepts, Hallowell may seem to have strayed far from the Algonkian kinship studies that he first published in 1928, but he would be the first to claim that this was a natural progression of interest.

The concentration of field work on one area or tribal group and the subsequent development of theory from this carefully controlled material are not uncommon in anthropology. The American Indian field lends itself to such an approach because it is practicably possible for American anthropologists to make repeated field studies. In all such cases, comparisons have either been implicitly or explicitly drawn between the non-European data and the Western way of life familiar to the fieldworker. Hallowell has been perfectly conscious of the importance and significance of this comparison. His intimate knowledge of the Ojibwa of the Eastern Woodlands of North America has thus enabled him to check up on some of the possible ethnocentrism involved in such new psychological areas as the Rorschach projective test and to make considerable contributions to the development and assessment of their application.

The book is divided into four parts, the titles of which indicate the points at which purely descriptive materials are introduced: I. 'Culture, Personality, and Experience'; II. 'World View, Personality Structure, and the Self: the Ojibwa Indians'; III. 'The Cultural Patterning of Personal Experience and Behavior: the Ojibwa Indians'; and IV. 'The Psychological Dimensions in Culture Change.' It is hardly necessary to add that anyone working on any aspect of these topics will find the book of inevitable value.

MARIAN W. SMITH

**Education and Anthropology.** Edited by G. D. Spindler. Stanford, Calif. (U.P.) (London agent: Cumberlege), 1955. Pp. 320.

**57** Price £2 5s.

This publication is a notable landmark in relating the systematic study of problems in education to recent research and studies in anthropology. The American Anthropological Association cooperated with the School of Education and the Department of Sociology and Anthropology of Stanford University in inviting certain anthropologists to contribute papers, which were then discussed by educators and other anthropologists at a conference held at Stanford in June, 1954. The emphasis was on the American school system, and especially on the current assumptions about the role of the schools in modern American culture. Some of these assumptions could well be transferred to conditions in tropical areas, as for example: 'The schools are confronted with the subtle and complicated task of helping children to grow up in a more or less disorderly social life, where almost all our institutions and practices are being rapidly altered, and in some cases superseded by new social inventions.'

The titles of the papers indicate the interest with which some anthropologists in the U.S.A. are turning their attention to problems in the field of education: Models for the Analysis of the Educative Process in American Communities; The School in the Context of the Community; Contrasts between Prepubertal and Postpubertal Education; Discrepancies in the Teaching of American Culture; Culture, Education, and Communications Theory. The early discussions showed that some at least on both sides of the conference table had at first an initial difficulty in using terminology and concepts which were readily understandable by all participants. On the other hand, as the Dean of the School of Education at Stanford said in his introductory paper, 'educators are reading more anthropological literature, and using more anthropological concepts and content.' Anthropologists and educators agreed that their thinking and deliberations were oriented primarily towards the 'teachers of teachers,' mainly because in the conference they were concentrating on the formulation of concepts and theories.

It was clear, as the papers were discussed, that the anthropologists present expected to get, in the words of one of them, 'new concepts about the relationship between the educative and the cultural process that would point to new kinds of research leads,' as for example in the field of cultural transmission. Many of the current concepts about culture, cultural values, and cultural integration appeared in several of the discussions, together with the current ideas of the teacher as an innovator, the educative process as a process of culture change, and the assumption of responsibility for planning and supervising a school system.

The importance of these papers for educators working in the tropics is without question, and it is to be hoped that the interest shown in this field in Stanford University will inspire universities in tropical areas to consider similar conferences.

MARGARET READ

## CORRESPONDENCE

**On Segmentary Lineage Systems.** Cf. MAN, 1956, 5

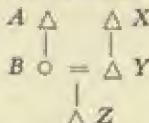
**58** Sir,—Dr. M. G. Smith complains in MAN that in the work of 'Evans-Pritchard and his followers' (whoever they may be!) 'misunderstanding of political systems, and definition of segmentary lineages in terms of such systems, has gone hand in hand with an unfortunate dichotomy between lineage and kinship.'

Dr. Smith has not himself understood the views he considers 'unfortunate.' It is probably true, if not entirely precise, to say that 'the theory of segmentary lineage systems . . . distinguishes sharply between the lineage principle and other kinship principles,' but it is misleading to say that 'definitions of political relations and action . . . provide the foundation for' that distinction. And again, though Professor Evans-Pritchard and others have admittedly pointed out 'the political primacy of lineage affiliation' in certain particular segmentary societies, that is something different from 'the analysis of segmentary lineages as a political form.'

In his study of the Nuer, as I understand it, Professor Evans-Pritchard showed that in Nuerland lineage groups and political sections tend to be associated and that two distinctive features of lineage groups—their persistence and their segmentation—are also characteristic of Nuer political groups. Having noted this structural congruity between lineage groupings and political groupings he further suggested that in Nuerland the connexion between the lineage structure and the political structure is to some degree causal: 'The lineage system is twisted into the form of the territorial system in which it functions' (*The Nuer*, 1940, p. 265). Nowhere did he define segmentary lineages in anything but general structural terms (*viz.*, as groups of people who view their relations both with other members of their group and with members of other groups in terms of unilineal descent). It cannot be Professor Evans-Pritchard who has analysed segmentary lineages 'as a political form,' for he took great care to distinguish between lineages and political units: 'Nuer lineages are not corporate, localized communities, though they are frequently associated with territorial units' (*op. cit.*, p. 203).

Relations between people in terms of unilineal descent may be said to have a political function in those societies where unilineal descent is thought to validate territorial groupings; but that is not to say that unilineal descent relations may not also be viewed, from another point of view, as kinship relations of the same order as relations of affinity or consanguinity, by virtue of which persons of one category regulate their behaviour to persons of other categories in ways that are not in any sense political.

Suppose that X is the father of Y who is in turn the father of Z, while A is the father of B who is the mother of Z:



All five may be said to be kindred. If Z resides in the hamlet of his father Y and his grandfather X, under the authority of the hamlet's headman, in obedience to a general rule that men belong to the political group of their father, then the relationship of unilineal descent linking X, Y and Z may be said to have a political function. But suppose further that Z is to be married, and that it is considered proper for X to contribute ten armshells to the bridewealth whereas A need only contribute two. In such a case we should say that the person related to Z by unilineal descent, namely his paternal grandfather X, stands in a different category of kin relationship from that in which his maternal grandfather A stands to him, this particular distinction having effect in the matter of bridewealth contributions; and then our interest in unilineal descent would not be primarily political.

May we be pardoned for wondering what it is about 'segmentary lineage theory' that so greatly puzzles some of our colleagues? It is interesting to note that Dr. Smith's contribution to MAN appears in the same issue as an article by R. F. Salisbury in which a thorough mastery of 'segmentary lineage theory' is applied without fuss, in a routine way, to a study of a New Guinea Highland society. Its

limited usefulness in that new context is demonstrated, and an interesting hypothesis about segmentary lineage and segmentary political systems is modestly advanced.

Dr. Smith makes a second general complaint. He thinks that a distinction between segmentary political systems and hierarchic political states is untenable. All political systems, he says, are both segmentary and hierarchic, segmentation being a 'political' matter concerned with 'determination of policy' and hierarchy being an 'administrative' matter concerned with 'the routine management and coordination of group affairs.' I am at a loss to understand Dr. Smith's use of words. There seems to me to be a very clear sense in which African kingdoms or principalities have hierarchic governments with specialized administrative machinery and Nuerland has not, and in that same sense there seems to have been some traditional machinery of government in the Trobriand Islands and none in Dobu. Surely Dr. Smith would not deny the truth of such statements. What is it, then, that he is concerned to deny?

When it is published in full, Dr. Smith's essay may perhaps by illustration show us in what sense he is able to believe that segmentary societies 'have this segmentary form and character by reason of the cross-cutting combination of both hierarchic and segmentary principles, the former administrative and the latter political, about which they are constituted.' As it stands, the statement is most obscure. Does it mean that the members of a political group at any particular level of segmentation within a segmentary political system have not only ways of initially deciding to act together for some particular purpose ('policy' in Dr. Smith's terms) but also ways of seeing to it that people subsequently conform ('administration')? If so, who would wish to deny that?

Balliol College, Oxford

MURRAY GROVES

**'Slash-and-Burn' Cultivation.** Cf. MAN, 1955, 144; 1956, 22

**59** Sir,—I deprecate the introduction of a new term as an unnecessary burden to the memory. You refer in your note to 'resorting to dog Latin or worse,' and the horrible German portmanteau *brandwirtschaft*, advocated in your January issue, is even worse than the dog Latin 'Londiniocentric' in your note.

G. H. S. BUSHNELL

University Museum of Archaeology and Ethnology, Cambridge

#### Note

The nonceword 'Londiniocentric,' having served its half-humorous purpose, is gladly withdrawn. As to Mr. Greenaway's advocacy (MAN, 1956, 22) of *Brandwirtschaft*—which is surely not a portmanteau word?—the fact that Professor Grahame Clark found it necessary to use this word so extensively is possibly the strongest argument for the adoption of a new word.

Apart from its other shortcomings, 'slash and burn' is not properly applicable in the numerous cases in Africa and elsewhere in which the slashing is dispensed with. Perhaps our correspondents have not fully appreciated the advantages of 'swidden/switthen,' some of which were implied rather than stated in Professor Ekwall's note: chief among these is its versatility, since, unlike any of its alternatives, it is equally available as noun, verb and adjective.

The strain upon anthropologists' memories is indeed great and constantly increasing, so much so that a comprehensive anthropological lexicon is an obvious need—or better an evaluative 'Modern Anthropological Usage' as trenchant as Fowler's work. However, the general adoption of one good term in place of several bad ones would clearly reduce the strain. Further opinions, from any part of the world, will be welcome.—ED.

**Hæmoglobin D Disease in a Sikh: A Correction.** Cf. MAN, 1956, 1

**60** Dr. Lehmann informs the Hon. Editor that, owing to an undetected error in the original typescript of his article in MAN, 1956, 1, reference was incorrectly made in paragraph 3 to 'the reduced ferro-hæmoglobin of C.' This should have read 'the reduced ferro-hæmoglobin D.'





(a) An early seventeenth-century portrait of a Princess of Zanzibar, who became the wife in 1604 of Sir John Henderson, with her maid.  
Photograph: National Galleries of Scotland, Edinburgh



(b) The exterior of the palace at Kilwa. Photograph: Public Relations Office, Tanganyika Territory

# THE CULTURE OF THE EAST AFRICAN COAST\*

IN THE SEVENTEENTH AND EIGHTEENTH CENTURIES  
IN THE LIGHT OF RECENT ARCHÆOLOGICAL DISCOVERIES

by

THE REV'D. GERVASE MATHEW, F.S.A.

Lecturer in African History and Archaeology at the University of Oxford

61 Since I first began fieldwork in East Africa in 1946 I have become convinced that many of the ruins on the Kenya and Tanganyika coast that were once ascribed to early mediaeval Persian settlements were built during the seventeenth and eighteenth centuries between the fading of the power of Portugal and the rise of Zanzibar. The sites are linked by the common use of blue-and-white Chinese porcelain of seventeenth- and eighteenth-century types and, though less commonly, of cheap glazed ware from the Persian Gulf. There are recurrent types of house, palace and mosque, pillar tomb and fortification. Dated inscriptions have been found in considerable numbers, especially at Pate and on Wasin Island. It was a composite culture, always at least nominally Islamic, influenced from south Arabia, from western India and perhaps from Portugal, but certainly deeply africanized and probably integrally African. The ruined towns represent a group of small island states, oligarchic in their social structure, using a currency of beads and rolls of cloth, and trading in ivory and in slaves.

A survey of the sites so far recorded may suggest the economic basis of their prosperity.

Kismayu in the present Italian Somalia seems to mark the northern limit of this Swahili culture; beyond lay Brava with its separate speech and the towns of the Ben Adir. From the thirteenth to the nineteenth century it was a curiously fluctuating<sup>1</sup> site within reach of the mouth of the river Juba. In the seventeenth century it was in decay, perhaps because the southward surge of the Galla road had broken the trade routes inland; this might also explain why the towns on the Coiama Islands had been deserted. The Lamu Islands had become the chief centre of Swahili culture, perhaps because a trade route that penetrated far inland up the Tana river intersected here the monsoon route to Arabia and India. During a survey in 1949 I noted five sites that can belong only to this period. Old Lamu is buried beneath the sands. Modern Lamu is a Zanzibari creation; so perhaps also was Shellal, but Taka on its creek among the mangrove swamps is rich in seventeenth- and eighteenth-century inscriptions, and so too is the ruined town of Pate. With these must be classed Faza, Siu, and Kitao on Manda Island, and on the mainland opposite, two sites near Mkunumbi and perhaps the older portion of Witu. The wealth of Pate was a proverb: it was said that there the nobles climbed by silver ladders into ivory beds. The chief literary dialects in old Swahili gained their names from Lamu and from the palace dialect of Pate, and by the eighteenth century Lamu had evolved its own intricate style in woodcarving and in many-coloured textiles.

\* With Plate E and five text figures. A paper read to the Royal Anthropological Institute, 2 February, 1956

Southward the next important seventeenth- and eighteenth-century site is Mambrui, which had succeeded Malindi by the outlet of the Sabaki river, then Mombasa Island where so much remains in palimpsest beneath the modern city, then Wasin Island, then Tanga Island. These three were primarily halting places on the coastal route as were the towns of Zanzibar and Pemba. The next two centres, Pangani and Kaule-Bagamoyo, were clearly created by trade routes that reached far into inner Africa. Then come the Mafia group of islands (in many ways

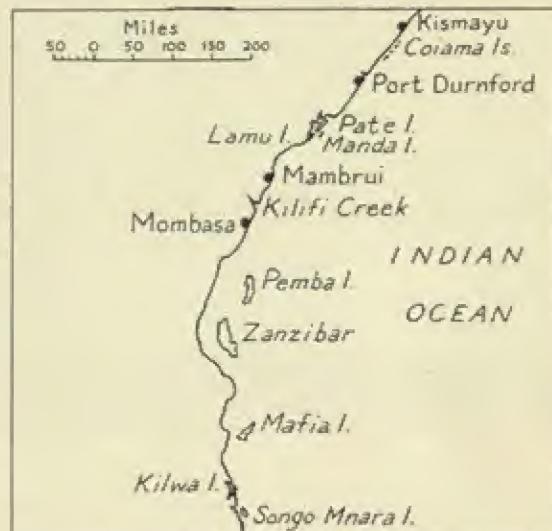


FIG. 1. THE EAST AFRICAN COAST

forming a parallel with the Lamu group), at the point of intersection between the monsoon route and the traffic down the Ruviji river. It was a famous centre for Swahili poetry, with Kua as its capital till it was succeeded by the Zanzibari creation of Chole.

It may be suggested that Kilwa Island paralleled Kismayu, the outpost of a culture in which it was not fully integrated; certainly European accounts of the late eighteenth and early nineteenth centuries suggest that it was a town of quite different pattern from those to the north<sup>2</sup>—reed huts with only two stone buildings in use, the castle and the King's House. The latter, according to the chronicle of Kilwa Kivinye, is a late eighteenth-century reconstruction, though probably within the shell of an older building (Plate Eb, fig. 2). Kilwa had only recovered slowly from its destruction in 1587; that it recovered at all was due primarily to the fact that it was the beginning of two overland routes, one leading to the Zambezi,<sup>3</sup> the other to Lake Nyasa.

It seems clear from this archaeological survey that the Swahili culture of the seventeenth and eighteenth centuries



FIG. 2. THE INTERIOR OF THE PALACE AT KILWA

*Photographs (figs. 2 and 3): Public Relations Office, Tanganyika Territory*

was primarily a town culture and that its prosperity depended largely on trade with inner Africa, presumably in ivory and slaves. I am inclined to believe that trade with inner Africa was a key factor in the prosperity of the mediaeval cities along the coast; its extent has been illustrated by Dr. Caton-Thompson's work at Zimbabwe<sup>4</sup> and possibly by Mrs. Leakey's finds at Hyrax Hill in Kenya.<sup>5</sup>

Some of the Swahili towns were built on sites that do not seem to have been previously occupied, but in most cases there were direct links with the far more highly developed civilization of the islands described by the first Portuguese adventurers.<sup>6</sup> These were links only.

At the beginning of the sixteenth century the Portuguese describe great walled cities with high stone houses, palaces and domed mosques ruled by powerful kings and inhabited by 'white Moors' as well as black. These white Moors who formed the ruling class were possibly partly Arab, partly Persian, partly Indian Muslim by origin. It is certain that they were Arabic in culture as is illustrated by the rhyming couplets on the many thousands of coins from

the Kilwa mint, by the Kilwa chronicle and by scattered inscriptions on the coast. It was a money economy. Over 20,000 coins of the mediaeval period have already been recorded from East Africa; predominantly from Kilwa, they include some hundreds from China, specimens from Persia and Mesopotamia, and to the north many from Cairo and Damascus. By the end of the sixteenth century the 'white' and 'black' Moors (*cf. Plate Ea*) were merging into a single people, inscriptions are in variants of Swahili and I have not been able to record a single coin between 1572 and 1810.

Two factors, the economic policy of Portugal, and anarchy on the East African mainland, had contributed to the transformation between 1507 and 1592. Portugal had deliberately altered the Indian Ocean trade routes for the benefit of Mozambique and Goa; the routes to the interior must have been broken as the hordes of Zimba under their Divine King pressed up from the south-west, while the 'Mosseguejos' and behind them the first wave of the Galla came down from the north. Both Kilwa and Mombasa were first sacked by the Portuguese and then destroyed by

the Zimba. Only in the Malindi area did much of the old culture still survive, for Malindi was the protected satellite of Portugal.<sup>7</sup>

From 1592 to 1631 there is evidence of a serious attempt at European settlement. Spain had now acquired all Portuguese possessions. Philip II built the great fortress at Mombasa, and there was considerable missionary activity



FIG. 3. EIGHTEENTH-CENTURY INSCRIPTION AT KILWA

and evidence for Portuguese settlers at Pate, Faza, Mombasa and Zanzibar. But East Africa conceived as part of the '*estado da India*' and administered from Goa and the settlements could only survive as long as Spain had control of the sea routes. That control was already threatened by 1610, the great rising of 1631 destroyed the most flourishing of the settlements, and the punitive expedition of 1635 can only have added to the devastation.<sup>8</sup> From 1637 till about 1790 the small island states were able to maintain a precarious independence as the power of Portugal faded southwards towards Mozambique, and the rising power of the Omani Sultans from the Gulf was checked sporadically by Persian pressure and by dynastic feuds.

The political history of this period is well documented. It is harder to discover the social and economic facts that underlie it. The sites were normally independent, though Pate would seem to have exercised some kind of hegemony from 1698 to 1754. They were at least nominally monarchic but it is curious to note how often their rulers were women—at Zanzibar in 1653 and 1696, at Pemba in 1756, at Pate in 1750 and 1773, in the Mafia Islands about 1790. There is some evidence that the succession could be matrilineal; Mwana Fatuma is stated to have succeeded her aunt Mwana Miveni at Zanzibar, and there is a reference to a Mwana bint Mwana ruling Tumbatu Island. Possibly these 'queens' succeeded to ritual functions, the guardianship of the Drum or of the Horn, but inscriptions as well as documents suggest that women held a more prominent place than would be normal in an Islamic society. Even the Decorated Ones, the *Wa Pambe*, the courtesans, seem to have possessed a recognized status.<sup>9</sup>

But true power would seem to have rested with ruling clans like the Nabhani at Pate or the Masrui at Mombasa together with rich traders affiliated to their groupings like the Hadhramauti stock of Abu Bakar ibn Salim or the half

Portuguese Famao at Siu. Inscriptions are providing a check on the oral pedigrees, probably conventionalized and lengthened during the Zanzibari period. It seems clear that here also a matrilineal descent was often especially prized.<sup>10</sup> It is this ruling class who are the 'Masheke,' the 'Mabwana,' the 'Mawaziri' of classic Swahili poetry, the 'Comptrollers of the Armaments of War' whom the author of the *Inkishafi* could remember in eighteenth-century Pate swaying their arms and arching their necks as the common people gazed at them. It is perhaps another un-Arab trait in this culture that the swinging of long necks and of many-jointed arms marked the conventional deportment of a noble.<sup>11</sup>

It is remarkable how closely recent archaeological discoveries have corroborated the descriptions of such poems as *Al Inkishafi*. In August, 1955, Sir Mortimer Wheeler rediscovered the deserted town of Kua on Juani Island in the Mafia which had been hidden by deep bush. Its sudden destruction by pirates from Madagascar with the stories of its last queen Muanzuan had become part of the folklore of the western Indian Ocean. Having collated five versions I would judge the most likely date of the sack to be approximately 1790, and this is supported by the chronicle of Kilwa Kivinje. It is probable that the site was continuously occupied; the coins reported from it are of the thirteenth, fourteenth and fifteenth centuries, and it is presumably the 'Coa' of the Portuguese record. It remains a uniquely

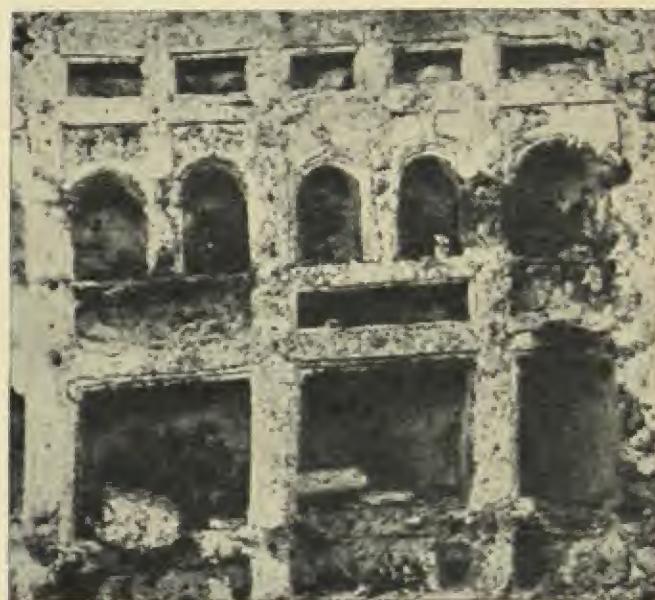


FIG. 4. MAIN ROOM OF A SEVENTEENTH-CENTURY HOUSE AT KUA, MAFIA IS.

*Rows of niches for porcelain are shown*

complete specimen of an island capital of the eighteenth century even if some of the buildings in it may be older. There is the small two-storeyed palace built of coral with its halls of audience and council<sup>12</sup> and close to it the nobles' houses, often elaborately decorated and with the lines of wall niches for the display of porcelain<sup>13</sup> (fig. 4), and the

walled courtyards,<sup>14</sup> four mosques, and as in all such sites the high phallic pillars that mark the dead (fig. 5).

Neither the origin nor the function of these pillars has yet been adequately examined. During the expedition to Port Durnford with Sir Mortimer Wheeler in 1955, I noted a high pillar on the south bank of the estuary about  $\frac{3}{4}$  miles above Bur Gao. It was far higher than any other I have found by the coast and oddly reminiscent of the lesser stelæ at Axum. It is not impossible that the pillars will be found to have their origin somewhere in this unexplored



FIG. 5. SEVENTEENTH- OR EIGHTEENTH-CENTURY PILLAR  
TOMB AT KUA

Found on Juani I. during expedition to the Mafia Is., August, 1955

area, perhaps influenced by the Ethiopian hagioliths. They occur, ornamented with early Ming porcelain, in what are clearly mediaeval sites; they pullulate in increasingly obvious phallic forms in the Swahili sites along the coast; I have never found any north of Port Durnford or south of the Mafia Islands.<sup>15</sup> Their function, perhaps their changing functions, should be left to the anthropologist. There is some slight documentary evidence that they were prayed to.<sup>16</sup> There is much archaeological evidence that sacrifices took place at them.<sup>17</sup> It is probable that the China ware with which they were increasingly ornamented had some magical significance.<sup>18</sup> They are a reminder of the non-Islamic elements in the religious beliefs of the seventeenth- and eighteenth-century Swahili.

It is unquestioned that the town Swahili were consciously Muslim. It is also unquestioned that their religion was deeply impregnated by trust in magic and by fear of spirits, not only spirits in the forest but spirits that walked the streets in the fourth hour.<sup>19</sup> It is not yet clear how these were propitiated. Perhaps it was by animal sacrifice—I have found a goat's skull perched on a tower at Songo Mnara, and, less conclusively, goats' skulls within Kua palace and Kitao fort. Perhaps on occasion it was by human sacrifice; split skulls of children have been reported from beneath foundations in Kenya and in Zanzibar. It seems

probable that these spirits could be held to materialize or to be in some sense material. The dread of the '*mumiani*,' the technique of the blood-sucking human vampire, is still potent on the coast. It is clear that in each community the *Ngoma Kuu*, the Great Drum, or the *Siwa*, the ivory horn, like that still preserved at Lamu, were something more than ritual objects. Perhaps they enshrined in some fashion not only the strength but the spirit of a people. But these are questions which an archaeologist may raise but only an anthropologist can answer.

The Swahili towns gradually passed into the control of the rulers of Muscat between 1792 and 1837; their prosperity, sapped by the development of new Zanzibar as a central *entrepot*, vanished with the trade in slaves and ivory on which it had depended. Now for the most part they are dead or dying. But much of their culture still survives by the ruined ports and in the fishing villages, and as far as I know it has never yet been studied in detail by a trained anthropologist.

There are three immediate needs if we are ever to gain a clearer knowledge of the coast in the seventeenth and eighteenth centuries. The first is for the publication of a volume of selected documents and inscriptions. The second is for the full excavation of a manageable small site, perhaps the Palace quarter at Pate, though this will be merely destructive unless a full report is published. The third is for anthropological fieldwork in a Swahili coastal community, perhaps Pangani or Mambrui or Witu. All three needs interlock. Africa can only be deciphered when history and anthropology are at least coordinated.

#### Notes

<sup>1</sup> Old Kisimayu seems to be represented by the scattered ruins at Gondal.

<sup>2</sup> Cf. 'Kilwa in 1812,' by Sir John Gray, *Tanganyika Notes and Records*, December, 1947.

<sup>3</sup> For the route from Tete to Kilwa in 1616, cf. the article by Sir John Gray in *Tanganyika Notes and Records*, June, 1948.

<sup>4</sup> G. Caton-Thompson, *The Zimbabwe Culture*, 1931.

<sup>5</sup> Cf. S. Cole, *The Prehistory of East Africa*, 1954, pp. 278f.

<sup>6</sup> E.g. Duarte Barbosa (trans. in Hakluyt Society, 1918); Joao de Barros, *Asia*, Vol. I. (Coimbra, 1930); 'The Voyage of Francisco d'Almeida' (printed in E. Axelson, *South East Africa*, 1940).

<sup>7</sup> Cf. J. S. Kirkman, *The Arab city of Gedi*, 1954.

<sup>8</sup> Cf. the detailed inscription on the outer gate of Fort Jesus at Mombasa.

<sup>9</sup> E.g. *Al Inkishafī*, v. 41, cf. Mwinyi Kaye wa Mwinyi Malenga, c. 1770.

<sup>10</sup> Cf. *Al Inkishafī*, v. 57, and *Takhwisa*, v. 28.

<sup>11</sup> 'Kumia Shungo,' 'Kumia Mbiru,' 'Mbiri Za Mikono,' cf. *Liongo na Manga*, v. 27.

<sup>12</sup> Cf. *Al Inkishafī*, vv. 39, 52.

<sup>13</sup> *Al Inkishafī*, v. 50. For the uses of porcelain in this period, cf. 'Chinese Porcelain in East Africa and South Arabia,' by Gervase Mathew, *Oriental Art*, July, 1956.

<sup>14</sup> *Al Inkishafī*, v. 52.

<sup>15</sup> The one exception, a broken hexagonal column above the dome of the smaller mosque at Kilwa, may be only for ornament.

<sup>16</sup> E.g. the case of 'Dom Jerominao Chingulia' accused of praying to his father's tomb at Mombasa before the rising of 1631.

<sup>17</sup> I have noted nine cases of animal bones and shattered porcelain found in association with tombs.

<sup>18</sup> For this suggestion I am indebted to my friend Greville Freeman-Grenville.

<sup>19</sup> Cf. *Mwana Kupona*, v. 45.

## ZANDE CLAN NAMES

by

PROFESSOR E. E. EVANS-Pritchard

*Institute of Social Anthropology, Oxford*

**62** When Azande are asked why a certain clan has a certain name they usually, in my experience, say that they do not know. Older men will sometimes, however, offer some explanation of the name, though not always with any certainty or even, I think, conviction; adding, maybe, that they have heard it from someone or that people say that it is so. It soon becomes evident that, in most cases at least, the stories they tell of why these names were given to the clans are examples of what Max Müller called 'disease of language,' stories invented by someone to explain the names, playing on the literal meaning of the words or of words to which their sounds bear a resemblance. The names are usually presented, however, as being nicknames, conventionally thought to have been given to the clans by the wives of their sons, bestowed on account of some characteristic or habit of their members. I give some illustrations.

The Akalingo clan are so called, it is said, because they used to behave badly to their fellows and kinsmen, not being able to abide that another should eat their food, and saying that when it was a matter of food or other goods, unless they were very close relatives indeed, they had no kinsmen. The last two syllables of their name, *lingo*, mean, when combined with the verb *de*, to cut kinship; and we may conclude that the story that this clan were mean with their kin derives from this purely verbal association. In doing so, we reverse the construction of the Zande etymologists, that people of a certain clan behaved in a certain manner and therefore got a certain name, by saying that a chance identity or similarity of sounds has led to an explanation of the clan name and consequently to the attribution of certain characteristics to its members in the past. The Abakundo clan, who in Zande tradition were their chiefs in ancient days before the ruling Avongara clan of today became dominant, are said, with little respect for the composition of the word, to have got their name because when they killed an animal they used to put it for two or three days in a granary, since they liked to eat meat when it was high (*kundo*). The Angumbe clan bear a name which is the same word as that used to denote a species of oil-bearing gourd, and therefore it is attributed to them that in the past they were exceptionally industrious cultivators and that they chiefly planted in their gardens this gourd, which they preferred to all other food. The Abalingi clan are said to be so called because their ancestors displayed great meanness (*lingi*) in the matter of food. The Angbadimo clan were noted for their dislike of their sisters' sons and their sons' wives entering their huts, for these relatives are in the habit of appropriating anything which takes their fancy. When a sister's son visited them they would sit in the doorway (*ngbadimo*) of their huts till he departed, to deny him entrance. Hence their name. The Abakpara were another

clan who are said to have got their name on account of their lack of cooperation with others. They wanted to keep all their affairs, especially in the matter of food, to themselves and to separate (*kpara*) themselves in their activities from others. In the case of the Ambale clan the last syllable, *le*, has caught the ear, and it is said that when members of this clan used to kill an animal they could not bear that anyone but themselves should take its intestines (*le*), of which they were very fond.

Probably all these derivations are to be regarded as fictions, fanciful attempts, not taken very seriously by anyone, to account for the names of clans, which, assuming that, as is possible, they originally meant something, meant something quite different from the meaning given them in these aetiological stories. That what we are considering is merely popular etymology is evident, apart from the violence done both to sound and grammar, from the manifest absurdity of some of the explanatory myths, wild guesses, as they would appear to be, at how names might have arisen. There is a clan called Abagua whose name, we are told, arose from their habit of killing in great numbers red grass rats, known to Azande as *agua*. People deceived them by telling them that these rats were not good to eat, so they threw (*ba*) them away—hence *Abagua*, the throwers away of red grass rats. Likewise the Abatiko clan were the first to set traps for small birds, but when they found in their traps a very tiny bird called *tiko* they did not take it home with them but threw it away: hence their name *Abatiko* (*a*, pl. prefix; *ba*, to throw; *tiko*, birds of this name). The *ba*, which in these two last examples is identified with the verb *ba*, to throw, and forms the second syllable in the names of a fair number of Zande clans, is presumably a plural prefix in the original languages of peoples who today form part of the Zande ethnic and cultural complex, the usual Zande plural prefix *a* having been joined to it.

The Akurungu clan were the first to beat wooden gongs and to spread news by this means. Now a wooden gong makes a sound which, it seems, resembles to the Zande ear the cry of the *kurungu* or blue crested turaco, a bird found in thick forest near streams. The old name of the Agiti clan was Agbembara, 'pullers about of elephant,' because when they killed an elephant they could not abide that anyone but themselves should approach the carcase, which they pulled and chopped about, struggling among themselves for the flesh. Some of these interpretations are very far-fetched, one syllable being emphasized and the rest ignored, sounds being forced into a resemblance they scarcely bear, or some meaning the words can be supposed to have being given a metaphorical value which makes some, if little, sense when used in reference to people. The Agbutu are so called from their habit of hiding (*gbu*) their

affairs from others; the Angali, because they played games dangerously like the play of a dog (*ango*); the Avukida, because they were always promising (*kida*) what they had no intention of carrying out; the Abadala, because they hoed their maize gardens where there grew a riverside grass called *dala*; and the Akowe, whose original name was Abananga, because of the unfaithfulness of their wives (*ko we*, to make fire by friction—a euphemism for sexual congress and hence for adultery). These explanatory stories are manifestly absurd.

Even those Azande who seldom failed to give explanatory commentaries on clan names when these were requested from them were sometimes beaten by names which neither mean anything nor can easily be distorted into meaningful sounds, for example, in the case of the Agbambi and Aubali clans. They then generally said that the names bore no meaning because they were originally clans of foreign origin, belonging to such peoples as the Amiangba and the Abarambo, and that the meanings of their names are either unknown to Azande or have become lost to everybody with the disappearance of the languages to which they belonged. The Zande language has, they said, become all mixed up with (*kpamiakpamia*) foreign tongues. This is a true enough observation, but it ignores the fact that a large number of clan names for which they put forward an etymology are the names of foreign clans and are unlikely therefore to have originated in the way suggested, since although that is what the words might mean in Zande the probability is that they are not Zande words at all. This is the case with a number of the clans I have already mentioned, and it is the case with others. The Ameteli are, it is agreed by all, a foreign clan; yet we are told that they got their name because they would say to a man with whom they quarrelled '*mi a ta li ro na kina gi mi ngbondo*', 'I will strike your head with my club.' So, because the sounds in the word Ameteli bear some resemblance to the pronunciation of the Zande words *mi*, I; *ta*, strike; *li*, head, this clan is credited with having been the first to shape clubs and use them in fighting. Another clan of *auro*, as Azande call all foreigners, are the Angbaga, and here, if we consider the phonetics of the word alone, the etymology appears more reasonable, for there is a Zande word *angbaga*, meaning 'hoops.' Hoops are made by twisting pliable withies into circles, and a game is then played with them. The hoops are bowled at great speed, and those along their course try to hurl spear-shafts through their centres. We are told that the Angbaga got their name through addiction to this sport. Another foreign clan are the Akpura. There happens to be a Zande verb *kpura*, meaning to cook together oil and vegetable (usually manioc) leaves. This clan is therefore credited with having invented this cuisine. There happens also to be a Zande noun *maka* which means a coil of metal wire such as in the past Zande women used to twist round their arms as an ornament, and so the foreign clan of the Avumaka are said to be so called because they invented this form of ornamentation—*vu* having to do service for *vo* or *voda*, to bind.

There are a number of other Zande clan names which begin with the plural prefix *a* followed by *vu* or *vo*, and it

is possible that the second syllable *vu* or *vo* was, as has been suggested earlier for the *ba* in other names, a plural prefix. This is a very involved linguistic question. What we have to note here is only that, just as when what follows the *ba* can by any stretch of the imagination be thrown it is taken to be the verb 'to throw,' so when what follows the *vu* or *vo* can by any stretch of the imagination be bound it is taken to be the verb 'to bind.' Two further examples must suffice. The Avundukura clan are said to have originated the custom of tying (*vo*) a little bottle-gourd (*ndukura*), containing oil, to their waists for purposes of toilet when going on long journeys. The second example is the ruling clan of the Azande, which Europeans have variously spelt Avongura, Avongara, Avongora, and in other ways, and as to the meaning of which they have speculated, not very convincingly. I was told by Azande a long and, to me, fanciful if not fantastic story about how a powerful bully called Ngora (or Ngara) was overcome and bound (*vo*) by a man who was consequently given the nickname of Vongora, the binder of Ngora, and whose descendants became known as the Avongora. The late Father V. H. van den Plas in his paper 'Quel est le nom de famille des chefs Azande?' (Congo, Vol. I, 1921, p. 9) played the same etymological game as the Zande etymologists, and, as he heard the clan name as Avongara, he put forward the suggestion that the name meant 'those who had "lié-la-force"' (*vo*, to bind; *ngara*, force).

It does not follow that in all cases, and necessarily, these explanatory myths, if they may be so called, are fictions, merely imaginative play upon words. In some cases the associations on which they are based may be sound. Thus the word designating all the true or original Zande clans and distinguishing them from assimilated foreign clans is Ambomu, and the statement that they are referred to by this word because their homeland, before they migrated eastwards, was the valley of the Mbomu river has much to commend it, for it is supported by a wealth of tradition and other ethnographical evidence. The word which means the opposite to Ambomu is Auro, strangers or foreigners. That literally it means 'easterners' may readily be understood in terms of the eastwards movement of the Ambomu. The explanation of the name Adio, by which the most easterly Azande are known among themselves, that this people originated on the banks of a river (*dio*) may also appear adequate. But these three names are ethnic rather than clan titles. A better example would therefore be the Abandogo clan. They have as totems the red field rat (*ndogo*) and the red pig (*zukubele*). They do not eat the flesh of either animal, and their body-souls are thought to pass into one or other of them after death. It does not try our reason too far to accept that, as Azande say is the case, it is because this clan have *ndogo*, red field rats, for totem that they are called Abandogo (or Andogo); though in view of what we have learnt of other clan names we might wonder whether the process could not have been the reverse—that they have acquired the rat as totem because its name resembled, or was the same as, part of their clan name, which may originally have meant something quite different.

It may be surmised from what has been said in this note that the etymology of clan names is a game anyone can play who is skilful in the use of words, and that therefore we might expect to find that different persons advance different interpretations. This is certainly sometimes the

case in matters of detail in the stories related, but the type of explanation is always the same. That there are different interpretations is evident from the writings of others about the Azande, for example Mgr. Lagae (*Les Azande ou Niam-Niam*, 1926, pp. 29-31).

## SHORTER NOTE

**Ghost Marriages among the Singapore Chinese: A Further Note.** By Mrs. Marjorie Topley, Hong Kong. With a text figure

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Since writing on Chinese ghost marriages last year (MAN, 1955, 35) I have had a further opportunity to be present at such a marriage and this time to obtain a photograph of effigies of the bridal pair seated together at a table round which the ceremonies on their behalf took place. I was unfortunately not able to be present for the actual 'wedding' which was carried out in the City God's temple, but witnessed the associated ceremonies which took place in a Dying House<sup>1</sup> in which I happened to be at the time. The circumstances were as follows.

A Cantonese boy aged about fourteen had been sent back to China by his family to continue his studies and while there had died under somewhat obscure circumstances. About a month after receiving news of his death, his mother had a dream in which her dead son appeared and told her that he wished to marry a Hakka girl who had just died somewhere in Ipoh, Perak. Since, the mother told me, he had given no precise details in her dream, the next day she called in a Cantonese female spirit medium and through her the boy gave the name of the girl together with her place of birth and age, and details of her horoscope which were subsequently found to be compatible with his.<sup>2</sup> The mother said that since the boy was obviously anxious to marry and his marriage would make things easier when her younger son came to take a wife<sup>3</sup> she had taken the advice of the medium and decided to arrange a ghost marriage. At no time was any attempt made to check the information about the girl given through the medium.

The total cost of the marriage was approximately \$200 Straits (about £23) and a priest was engaged to see to the necessary arrangements. He was one of a small group of professionals belonging to a Cantonese branch of the Cheng I school of Taoism who earn their living in Singapore by performing at funeral ceremonies and at Cantonese occasional rites.<sup>4</sup> The priest arranged for the 'wedding' ceremony to be held in the temple, hired a room at the Dying House, bought or made all the necessary paraphernalia and together with his troupe of colleagues and disciples performed all the appropriate ceremonies.

The ceremonies in the Dying House started at 8.30 a.m. and throughout the day consisted in the chanting of Taoist sutras. At 11.30 a.m. the effigies of the bride and groom were put into a trishaw and transported to the temple for the 'wedding' from which they returned an hour later. In the evening post-mortuary ceremonies of the usual Cantonese kind were held for the pair.<sup>5</sup> All the ceremonies were attended by the boy's parents and a brother and two sisters who were attending English schools in the Colony.

The photograph (fig. 1) shows the effigies seated side by side before a table on which a marriage feast has been laid. Both effigies and food are similar to those used in ceremonies associated with the dead. The heads of the figures are made of *papier mâché* and the bodies of paper stretched over bamboo frames. The couple are dressed in real clothing, only the bridal headdress and the

groom's hat and bow tie are made of paper. A wad of 'hell bank notes,' imitation money notes of large denominations, is stuffed into the groom's top pocket. On each side of the pair are bamboo and paper servants, the bride's bearing a cup and the groom's (out of the picture) a real packet of cigarettes of a popular brand. In front of the servants stand *ling* or *lung p'ai*, temporary bamboo and paper soul tablets which are burnt after the ceremonies. In front



FIG. 1. GHOST MARRIAGE FEAST IN SINGAPORE

of the groom is a photograph of the boy taken just before his return to China. Ranged round the room were various life-sized objects of bamboo and paper made for the use of the couple in their new spirit-world home. Notable among them were a dressing table complete with silver-paper mirror, a set of an 'aluminium' table and six stools, a large money safe, a refrigerator and several trunks of paper clothes and rolls of cloth. Outside the door in the street stood a large American-type bamboo-and-paper car.

At dawn, after the ceremonies were over, all the paper articles, including the bride and groom, were taken outside into the back yard. The couple were placed inside their car and everything was then dispatched to the other world by flame.

According to the Taoist priest in charge of the day's activities Cantonese ghost marriages are still by no means rare in Singapore and he has been engaged to perform them by people, mainly women, of various occupations and income.

### Notes

<sup>1</sup> Dying Houses, or less harshly in Chinese, *Ta nan kuan*, Houses of Big Difficulties, are institutions catering for the needs of the dead

and dying. Upstairs there is usually a sick room where people may die, and, occasionally, recover. Downstairs there is a mortuary and a room which is rented out by the day for post-mortuary rites.

\* Horoscopes of those intending to marry are compared for compatibility before betrothal.

<sup>3</sup> Sons in a Chinese family should be married according to seniority. Cf. MAN, 1955, 35.

<sup>4</sup> 'Some Occasional Rites performed by the Singapore Cantonese,' J. Malayan Branch Roy. Asiatic Soc., Vol. XXIV (1951), Pt. 3.

<sup>5</sup> 'Chinese Rites for the Repose of the Soul,' J.M.B.R.A.S., Vol. XXV (1952), Pt. 1.

## REVIEWS

### GENERAL

**The Interpretation of Dreams and On Dreams.** By Sigmund Freud. The standard edition of the complete psychological works of Sigmund Freud translated from the German under the general editorship of James Strachey. Vols. IV and V. London (Hogarth Press and Inst. of Psychoanalysis), 1953. Pp. xxxii, 751. Price £36 for the set of 24 volumes.

The work covered by these two volumes was regarded by Freud himself as his most important contribution; the new edition is the most comprehensive so far, and will probably be regarded as a classic for many years to come.

All eight prefaces written by Freud to the previous editions of this book have been included and every alteration of substance made by him since its first issue has been noted. In addition to the bibliography listing every work referred to in the text and in the numerous footnotes, this edition contains a special bibliography of other books on dreams published, mainly in German, before 1900. The main body of the translation is based on the eighth (1930) German edition, which was the last published during Freud's life, but these volumes cover a much wider ground than any of the previous editions. The clarity of style of the English translation is quite unusual for a book which was originally written in German—this is certainly partly due to Freud's clarity of thought, but the translators must be congratulated on their own contribution; as a result we have a book which is written in a better style than many original publications in the English language.

Quite apart from its appeal to psychoanalysts and historians of psychology, this edition has a particular merit for workers in related sciences. It presents a method which has led to great advances in psychiatry and psychology and has influenced many workers in sociology and anthropology. The development of a close link between psychoanalysis and anthropology was prophesied by Freud in his Preface to the Third Edition. Elsewhere (p. 685) he writes: 'Dream-symbolism extends far beyond dreams: it is not peculiar to dreams, but exercises a similar dominating influence on representation in fairy-tales, myths and legends, in jokes and in folklore. It enables us to trace the intimate connexions between dreams and these latter productions. We must not suppose that dream-symbolism is a creation of the dream-work; it is in all probability a characteristic of the unconscious thinking which provides the dream-work with the material for condensation, displacement and dramatization.' This edition is also extremely illuminating on Freud's habits of mind, on the progress of his theories, on the way he tested his hunches and developed or abandoned his formulations. Each science has its own methods, its own gadgets and peculiar techniques; but the inquiring habit of mind of a creative thinker and researcher cuts across all technicalities. This edition adds nothing new to what is known of Freud's theory of dreams; but it throws more light on Freud as a man.

T. G. GRYGIER

**New Directions in Psycho-Analysis.** Edited by Melanie Klein, Paula Heiman and Roger Money-Kyrle. London (Tavistock Publ.), 1955. Pp. xiii, 534. Price £1 18s.

This volume consists of 21 essays by various psychoanalysts whose views have been influenced by Melanie Klein's work. The first section is devoted to papers in clinical psychoanalysis, while the second part contains discussions of psycho-analytical insights on social processes, aesthetics, literature, and ethics. In this review we will be concerned mainly with the relevance of these contributions to the social sciences.

One cannot help but be impressed by the fertile imagination of

most of the authors and the amusing combination of naïveté and presumption that allows them to disregard almost all previous research in academic psychology, sociology and anthropology. In addition, most of the discussions dealing with social and aesthetic processes are characterized by a consistent lack of appreciation of group phenomena. Thus, in such papers as 'Psycho-Analysis and Ethics' (R. E. Money-Kyrle), 'A Psycho-Analytical approach to Aesthetics' (Hanna Segal), and 'Social Systems as a Defence Against Persecutory and Depressive Anxiety' (Elliott Jaques), the analyses and explanations rest heavily on personality patterns that are integrated during the first year or two of life. Here is a form of psychological determinism that neglects not only group phenomena but also the possibilities of personality changes in adult life (except, of course, by undergoing psychoanalysis). A few concrete examples may help to clarify these statements. Money-Kyrle, in his paper on ethics, sets up a classification of personality types; the humanist personality being the most mature and healthiest. He then states, 'I do not think, for example, that a humanist politician could tolerate the guilt of attacking a comparatively harmless neighbour, as Germany did in 1939, or of abandoning a friendly one, as Britain did in 1938.' He postulates an oversimplified deterministic relationship between personality types and complex institutional behaviour. Elliott Jaques in his analysis of conflict in an industrial plant, explains the workers' ambivalence towards management on the basis of the paranoid splitting mechanism used during the first few months of life in order to separate good and bad objects. 'Bad management' is perceived in this way because the working men project their own hostile impulses into management.

For all its theoretical shortcomings and tortuous analogies there are some stimulating ideas that are potentially fruitful for social science research. Marion Milner in 'The Role of Illusion in Symbol Formation' furthers our understanding of the origin of symbolism and some of its social and psychological functions. Her discussion raises some interesting questions about the relationship between child-rearing practices in a society and the nature of the symbolism found in its rituals. Jaques's paper on social systems contains suggestive implications for understanding such phenomena as acculturation and social innovation. He demonstrates that social institutions are used by its members to reinforce individual defence mechanisms in order to allay anxiety. Change involving a restructuring of social relationships may be resisted or cause considerable tensions for reasons not fully known to the participants and not obvious from superficial knowledge of role relationships.

In conclusion I should like to point to three major shortcomings of these studies that seriously limit their potential contributions to the social sciences. First, there is but little concern for methodological rigour. Psychoanalysis has reached a stage of development where it would not be premature and unduly hampering to devote more attention to careful research methods. In the United States, John Whiting has made a start in this direction in his cross-cultural studies of the relationships of child-rearing practices to theories of disease and cure. Secondly, it would broaden the scope of these essays if the authors gave more consideration to the plasticity of the adult personality and to the influence of group factors in explaining individual and social behaviour. Indeed, people such as Kardiner, Horney and Fromm have demonstrated the fruitfulness of psychoanalytical insights for the social sciences. Finally, it is unfortunate that there was little attempt to outgrow a narrow sectarianism and integrate the findings with pertinent academic research.

SEYMORE PARKER

**Ways of Mankind: Thirteen Dramas of Peoples of the World and How They Live.** Edited by Walter Goldschmidt. Boston  
66 (Beacon Press), 1954. Pp. vi, 212. Price \$3.75

At a conference of educational broadcasters held at the University of Illinois in 1950 it was decided, largely as a result of the efforts of Professor Robert Redfield, to try to put over a programme of social anthropology, and it was eventually put over in the form of

the series of short radio dramas of which this book is composed. They are intended to bring home the fact that Orientals and Savages act differently from White Americans largely because they have different ideas of right and wrong. The little dramas seem well suited to this purpose. The *dramatis personae* are mostly Amerindians, but Ancient Greeks, Chinese and Aranda also take part.

RAGLAN

**Urgeschichte des österreichischen Raumes.** By Richard Pittioni. Vienna (Deuticke), 1954. Pp. 854, 536 illustrations. Price  
67 D.M. 65

Austria, though a small and poor country, occupies a key position at the junction of the Middle and Upper Danube basins and is traversed by the most important routes linking northern Europe with the Mediterranean. Moreover, the copper and salt deposits of the Austrian Alps were exploited in prehistoric times, and the mines not only brought trade goods to Austria, but also, having been carefully studied by the author and others, afford the most vivid and complete picture of Bronze Age mining in the world. As a result Austrian soil has yielded a rich and varied assortment of finds from palaeolithic to Roman times that have far more than local importance. Pittioni's exhaustive and lavishly illustrated account of them will be indispensable for all students of European prehistory. These must indeed be grateful to the publishers for enabling so competent an author to present these precious data so completely. Indeed in no country save Denmark has the material been published on a comparable scale.

Yet, apart from the mines, little of the Austrian material has the same direct human interest as that described by Brondsted in *Danmarks Oldtid*. With the same exception, few Austrian sites have been excavated by modern methods. For domestic architecture, as well as for the chronological framework, Pittioni has generally to rely on better observations from adjacent lands. Minute descriptions and detailed analyses of flint types, ceramic forms and decorations inevitably occupy a large proportion of the 854 pages and make heavy reading for non-specialists. Even the specialist must learn Pittioni's reformed terminology. *Lithikum*, *Keramikum*, *frühes*, *mittleres* and *spätes Metallikum* replace the Palaeolithic, Neolithic, Bronze, Hallstatt and La Tène periods respectively (this being so there is less objection to the use of Reinecke's terms, 'Hallstatt A' and 'B' for the latest phases of the European Bronze Age). The chorological classification follows the hierarchy of *Welt*, *Kultur*, *Gruppe* and *Typus*. None of these concepts can be expressed precisely by any current English equivalent and Pittioni's own usage may not be quite consistent: he writes of a *Badener Kultur* within 'die nordische Welt,' but of the *Glockenbecherkultur* and the *Aunjetitzkultur*; the *Urnenfelderkultur* in the *Hallstatt-A-Horizont* falls into *Baierdorf-Velatitz*, *Hötting-Moritz*, and other *Gruppen* while in the *Hallstatt-B-Horizont* we have the *Typus Stillfried*, *Linz-St. Peter* and so on within the *Hallstattkultur* proper. English students will hardly welcome having to learn two Austrian place names to designate a single *Gruppe* or *Typus* and may easily confuse *Typus* with type.

Within this framework the author describes objectively with abundant illustrations and full documentation the development of human culture within the present frontiers of Austria from the last interglacial to the absorption of the area into the Roman Empire. The 536 figures bring together all the important documents and groups of finds that have hitherto been dispersed through a hundred or so volumes or even unpublished. We must mention with particular gratitude the reproduction of those crucial finds that have previously been described either inadequately or in quite inaccessible local periodicals: 'Middle Aurignacian' and Gravettian flints from Willendorf (Ebner's pit); the Baden pottery from the eponymous site; several copper shaft-hole axes attributed to the same '*Gruppe*'; the remarkable grave at Föllük of an adult and child accompanied by skeletons of two mares, a cow and a calf, goats and a ewe attributed to the Single Grave Culture; the grave groups from Oggau attributed to the later phase of the *Glockenbecherkultur* and to the *Wieselburg-Gruppe* of the *frühbronzezeitliche Flachgräberkultur*; the Late Bronze Age hoards from Drassburg, Haslau-Re-

## EUROPE

gelsbrunn, Schönberg and Seeboden (Carinthia); and the 'hearse-grave' at Worschach (Styria)—one of seven cremations known in Central Europe with horse-trappings of the earlier, 'Hallstatt A' phase of the urnfield cultures. On the other hand the scientific value of the illustrations (with a few exceptions) is gravely impaired by the absence of scales; sometimes (e.g., figs. 193, 364-5) even the objects on the same plate are not all on the same scale.

In a short review only a few special points can be noted. Pittioni espouses (p. 51) the view that Upper Palaeolithic blade cultures developed parallel in the East Mediterranean and West European areas and spread from the latter to Central and East Europe in contrast to the thesis advocated by Garrod in 1938. Without explicitly committing himself to accepting the pleistocene age of the wheat grains collected by Muhlhöfer in the arctic rodent bed of the Merkenstein cave (devastatingly criticized by the German botanist Schiemann even in 1940), he envisages the rise of an autonomous *Keramikum* (food-producing economy) in Temperate Europe under the influence of the post-glacial climatic optimum. Transverse arrow-heads and atypical microliths from a few Danubian I (but also II) sites would constitute links between Neolithic and Upper Palaeolithic traditions. The Baden and Mondsee cultures (*Gruppen*) are 'expressions of the interaction of Nordic [not specifically Baltic] and Danubian components in the north and east sub-alpine zone.' Leopoldsdorf with its segmented faience beads like the Oggau *Typus* is older than Reinecke's Bronze Age A1, following immediately on the Bell-beaker phase. No overlap is admitted between Reinecke's A and B; the Boheimkirchen *Typus* (correctly equated with Věteřov in Moravia) would still be Early Bronze Age (but that leaves a gap before the appearance of the urnfields, and at Bludovice the Velatice urnfield group seems to follow immediately upon Věteřov remains). Pittioni assigns even those bronze types from Baierdorf and Velatice that Reinecke had taken as typical of his Bronze Age D to the Hallstatt A horizon though he distinguishes in the latter an earlier group of graves without fibulae and a later group containing Peschiera and Spindlerfeld brooches. Now the last-named admittedly still fall within Montelius III—the closing phase of the 'Earlier Bronze Age' of Danish prehistorians; its equation with the later part of Hallstatt A leaves painfully little in Central Europe to correspond to Montelius IV and V!

V. G. CHILDE

**Les Civilisations néolithiques de la France dans leur Contexte européen.** By G. Baillaud and P. Mieg de Boofzheim. Paris  
68 (Picard), 1955. Pp. xv, 244, 96 plates. Price 1,900 francs

The authors have most admirably carried out their intention in writing this book: to make available an up-to-date (1953) survey of the neolithic and 'chalcolithic' cultures of France, which may serve equally as a stimulus to research and as a handbook for beginners. They have rightly insisted (evidently in the face of criticism) that it is precisely now that such a preliminary synthesis can be most useful. Inevitably, future research will necessitate some revision of the outlines now presented; what is much more important is that the results of such future work will no longer be interpreted within the confines of a restricted area of France, as has so often been the case hitherto, but in terms of the prehistoric sequence in Western Europe as a whole.

The authors have dealt succinctly not only with the major neolithic cultures and their regional facies within France, but also with their counterparts in neighbouring countries. They begin with an introductory chapter which sets the scene with a brief discussion of archaeological method (in which they follow Professor Childe), of routes of diffusion, and of the indigenous mesolithic groups. Of

particular interest is the evidence for long survival of mesolithic flint-working traditions which, according to the authors, may be detected in many neolithic and later cultures, suggesting that acculturation rather than replacement of population was the normal result of contact.

Chapters II-IV deal with the three main neolithic groups distinguished in France: Danubian, Western Mediterranean (really limited to what Professor Piggott has recently classified as *la civilisation de Châteauneuf*) and non-megalithic West European (Piggott's *civilisation de Chassey*; cf. *L'Anthropologie*, Vol. LVII, 1954). Each culture and local facies thereof is described in a uniform manner and the existing evidence presented, always in the same order, for lithic industry, ceramics, use of bone and antler, ornaments, dwellings, mode of life, graves, geographical extension, origin and chronological position.

Two chapters (V and VI) deal with 'chalcolithic' cultures, on the grounds that in France the 'Chalcolithic' is marked not only by the introduction on a small scale of metal, but also by diffusion of megaliths, religious representations of Mediterranean origin, the Beaker complex, the Corded-ware complex, and by the growth of population and trade connexions—phenomena which all appear more or less simultaneously.

A heroic attempt has been made to illustrate together all the types of artifacts, including tomb plans, which belong to individual groups, and to place the plates of drawings and maps opposite the appropriate text pages wherever possible. But the quality of the draughtsmanship is variable and since it was necessary to copy from existing illustrations, the value of some of the sketches is doubtful; a few are totally incomprehensible without reference to the text and scales are often not uniform or lacking altogether. I. F. SMITH

**Inventaria Archæologica. An Illustrated Card-Inventory of Important Associated Finds in Archeology. Great Britain, First Set. 69 Grave Groups and Hoards of the British Bronze Age. Edited by C. F. C. Hawkes. London (Garraway), 1955. Price 10s.**

This British set of ten cards forms part of an ambitious international card index of important associated archaeological finds, which it is hoped will overcome the major difficulty of archaeological literature, that so much important material is hidden in obscure periodicals. It is sponsored by the International Congress of Prehistoric and Proto-historic Sciences and has the backing of the Council for British Archaeology. The chief editor and initiator of the series is Dr. M. E. Marien, of the Musées Royaux d'Art et d'Histoire, Brussels, and he is supported by a distinguished group of editors representing fifteen countries, among whom Professor Hawkes represents Great Britain.

The scheme envisages the publication on quarto Bristol board cards of the more important associated finds, since it is by associations that dating systems and correlations can most satisfactorily be worked out, both on a national and on an international basis. Five parts have so far been issued by four countries, beginning with groups which are crucial to the understanding of the prehistoric dating of the issuing country. Each card bears illustrations, title details and reference numbers on the front, and a considerable amount of information on the rear, following a standard classification. In this way a great deal of fundamental material will be brought together in a convenient and accessible form. Moreover, the separate cards allow both easy comparison and filing to suit the interests of the user. Dr. Marien hopes that suitable finds will in future be illustrated in a form appropriate for immediate incorporation into the *Inventaria Archæologica*.

Of the fifty cards so far issued the British group is outstanding. Most obviously, the illustrations in the British set are the best drawn and the clearest, though the printing (photo-lithography?) has made the lines slightly fuzzy. The information is printed in 8-point type, which is large enough to be easily readable, but small enough to be economical of space, allowing a really good account of the specimens to be included without running on to extra sheets. The British set is the only one to supply a title page and a contents card. (It is hoped to issue complete indices to the work at every 250 or 500 cards.)

The eight finds included are all of the Bronze Age, all are now in the Ashmolean Museum, and five are from the Oxford region. They are a B beaker burial with arm-guard, dagger and knife (from Dorchester-on-Thames), a B beaker burial with gold basket earrings, and tanged and barbed arrowheads (from Radley); a cremation burial with knife, awl, segmented faience bead, three amber and ten shale beads (from Radley); the Crediton, Devon, hoard of two rapiers and two palstaves; the Leopold Street, Oxford, hoard of ten palstaves and a socketed axe of unusual form; the Burgess Meadow, Oxford, hoard of a palstave from the same mould as seven from Leopold Street, two spearheads, a knife, a chisel, a hammer and an ingot; the previously unpublished hoard from Barton Bendish, Norfolk, of a knife, two palstaves, a quoit-headed pin, five bracelets, two torcs and two rings; and the Stoke Ferry, Norfolk, hoard of two swords, a unique shape, a halberd, two spearheads and fragments of two others.

This is a very satisfactory start covering a reasonable range of typical Bronze Age associations. Some would have liked to see site plans on the first three cards, but there can be no complaints otherwise. This is a new and most useful tool which should lead many to look into the original publications and make further discoveries for themselves. The field still untouched is so enormous that future parts will be awaited with keen expectation.

FRANK WILLETT

**The Isle of Lewis and Harris: A Study in British Community. 70 By Arthur Geddes. Edinburgh U. Publ.—Geogr. & Sociol. No. 2. Edinburgh (U.P.), 1955. Pp. xvi, 340, bibl., index, 12 plates, 13 text figs. and maps. Price £2 2s.**

This book is conceived as a study of the geographical, economic, social and cultural development and potentialities of the Island of Lewis and Harris. To an anthropologist this, the largest and most northerly of the Outer Hebrides, offers attractive material. A form of seasonal transhumance (the shieling system) survives in parts of the island, while the coastal *machair* is still in some districts common land, divided periodically before it is cultivated (run-rig). The author estimates that in 1947 40 per cent. of the homesteads were 'black-houses.'

It cannot be said that Dr. Geddes has grasped the opportunities which such material offers. His account becomes an attempt to prove the superiority over all other types of social organization of the 'work-team,' a unit which the author identifies with the conjoint-tenant farms which in Lewis were broken up by 1830. The economic and social ills from which the island has suffered in more recent times are attributed to the decline of this 'work-team,' while conditions under the joint holdings are in contrast idealized by carefully selected accounts. We are provided with no detailed material on which this Arcadian picture is based, if indeed such data are available, while in describing the contemporary situation the author confines himself to airy sociological generalization with none of the casebook exemplification which is a *sine qua non* of an adequate anthropological study.

Dr. Geddes's description of the traditional spiritual culture is very largely a synopsis of Alexander Carmichael's classic *Carmina Gadellica* (*Ortha nan Gaidheal*), but in any case much of this material comes from Barra and other Roman Catholic islands, where conditions are very different. Even the geographical section contains much that is dubious: 'To us now, as to the Islesmen, their Island home is no mere fringe, but a heart of Britain' (p. 4). We are told that 'different communities [in Lewis and Harris] are distinguished by marked racial types or strains' (p. 104), yet we are given none of the quantified findings on which this statement is based. Sociological problems are disposed of by a mystical legerdemain: 'The paradox of loyalty to one's fellows with loyalty to a lord, of stoutly equalitarian thinking bound up with fervid admiration for an aristocracy, is nowhere more marked than among the Gael' (p. 11).

The writer is all too ready to cite the titles of his own work in fields far removed from that under review (cf. the footnotes on pp. 108 and 173), while we are also subjected to his views on such irrelevant topics as the Bible and its purpose, and his grandfather's military reputation (p. 156, note). His approach is perhaps best typified in this sentence: 'The fascination of Gaedic community is,

first, that its elements stand out so clearly, and, secondly, that links were forged in clanship which made its living chain, though heavy, a thing that shone with pride, and beauty too' (p. 134).

It is greatly to be regretted that a university press should waste its resources subsidizing in such a lavish manner—since the format and illustrations are admirable—the publication of such nonsense.

IAN WHITAKER.

**Neue Marksteine: Drei Abhandlungen aus dem Gebiete der überlieferungs-gebundenen Kunst.** Veröffentl. d. Österreichischen Museums für Volkskunde, Vol. III. By Karl Spiess. Vienna, 1955. Pp. v, 132, 28 plates, 15 text figs.

71 In the first treatise on traditional art, the author, a well-known authority on this special subject, differentiates between two types of the 'Tree of Life': (1) the vine growing out of a double-handled vessel, with birds picking at the grapes; (2) a tree, with the Water of Life gushing forth from a rift in its trunk, and birds sitting in its crown. The vine and double-handled vessel, outstanding symbols of the Hellenistic cult of Dionysos, were adopted by Early Christian art. Grapes picked by birds appear, e.g., on seventh-century English High Crosses (p. 6). The double-handled vessel is often represented on medieval paintings of the Annunciation, embroideries and carvings, and served also as a pattern for ceramics. The second type,

influenced by Indian notions, originated in Persia. European ecclesiastical carvers and embroiderers copied Sasanid silks, Persian rugs and miniatures. After 1500 the Persian 'Tree of Life' survived in the form of ornaments for rural wedding costumes and decorations.

The second treatise deals with the Magna Mater. Recent excavations in Russia have brought to light a large number of female idols similar to those of the third millennium a.c. The clay of the Russian idols, mixed with corn and roughly ground flour, proves their fertility character. Herr Spiess reminds us of Demeter, holding ears of corn, and invests with a new and deeper meaning the fifteenth-to-eighteenth-century Austrian and Bavarian paintings of the Virgin Mary, dressed in a cloak of corn ears.

In his last contribution, 'The True Face of Christ,' the author refers to the Abgar letter, an amulet, formerly widespread in England. Among various copies of the 'true portraits' he mentions nineteenth-century drawings by Thomas Heaphy the Younger and a painting by Emanuel Zane (Italian-Byzantine school, seventeenth century), preserved at Buckingham Palace. According to M. Rickert, 'the head of Christ at Winchester might well have furnished the model' for the heads of Christ by Matthew Paris. In Continental folk art the representation of Christ's Face, miraculously stamped on the Kerchief of St. Veronica, remained popular until the nineteenth century.

ELLEN ETTLINGER.

## CORRESPONDENCE

**Mixtec Codical Art.** Cf. MAN, 1956, 38

72 Sir.—In the summary of his recent lecture, Dr. Dark leaves several unanswered points, on which it may be useful to amplify his notes.

(1) The inspiration of the Mixteca-Puebla complex was due to the Toltec confederation, which the Mixtec always claimed to have been connected with their own history. A date for the fall of Tula accepted by some of the modern Mexican scholars is now A.D. 987. This accords well with Ixtlilxochitl's history book, but it is based on reasoning from the dates of the Mixtec documents.

(2) The dating of Mixtec histories is really very simple. As no family is likely to produce more than three generations within a single calendar round of 52 years, it becomes easy, when counting the generations in these strictly genealogical documents, to check within which calendar round any given year must fall.

(3) The nature of the codices of the Borgia group was established as definitely magico-religious, and extensive commentaries upon most of them were published by Eduard Seler half a century ago. I may add that there is no good reason for ascribing any of them to the Mixtec themselves. Two are definitely of Aztec origin. Of the others there are at least two distinct tribal groups represented.

(4) Codex Selden is a continuous document, and all the people in it are linked through ties of family relationship, not only those of the last eleven pages. The exceptions are foreign chiefs shown captured in war, and the two supernaturals, the Creator spirits.

One would wish to see a list of dates and genealogical notes obtained by Dr. Dark's most interesting statistical methods, for comparison with the Mixtec dates obtained by Dr. Caso and others from a direct reading of the material painted in the codices.

West Molesey, Surrey C. A. BURLAND

**Rock Gongs.** Cf. MAN, 1956, 23

73 Sir.—I would like to add some examples from Brittany to Mr. Bernard Fagg's paper on rock gongs in Nigeria.

In 1952 I visited two 'pierres sonnantes' at St. Bieuzy. Both are of bluish-grey stone and rest on modern cement pedestals. The first is said to have been struck by a pebble of quartz whenever St. Gildas wished to call his disciples and parishioners. This stone, in St. Gildas Oratory, is of irregular shape, approximately 28 inches long and 7 inches thick. Several corners had been chipped off. Two quartz pebbles were lying on its top and when struck the stone sounded like a bell. St. Bieuzy's stone is kept in the parish church. Its surface is 35×18 inches, and it is 14 inches thick. Of the two pebbles: a small one seemed to be used frequently; but the other, a large, beautiful quartz looked like a show piece and had no traces

of being used at all for striking the stone. I thought the sound of St. Bieuzy's stone was more metallic.

At Plouescat I was told about a 'pierre sonnante' quite near the coast: a natural rock, one part resting on top of the other. Near the port of Le Guildo I found a number of ringing rocks, almost in reach of the tide.

All these stone gongs have been described by Paul Sébillot and G. Guéniñ.

Oxford E. ETTLINGER.

**Multiple-Stemmed Pipe Bowls.** Cf. MAN, 1955, 6

74 Sir.—My attention was attracted by Dr. M. D. W. Jeffreys's recent note, describing multiple-stemmed pipes from the Ibo of Aguleri. In 1947, during a short visit to the Chami Indians of the Chocó District in Western Colombia, I was able to acquire two multiple-stem pipes of clay, one of which was made in my presence by a Chami woman. The object has the shape of a small cooking vessel with a flattened base and stands about 3 inches high. From its lower part protrude four clay tubes. I was told that this pipe is smoked during certain agricultural rites by four men lying on the floor, but I was unable to secure any further information on its use. The local term *chirumbé* applied by the Indians to this pipe seems to be related to the word *churumbela*, a rather obsolete Spanish term for pipe used occasionally by the Mestizos of some parts of lowland Colombia.

The Chami and other tribes of the Chocó District have been in contact with Negroes ever since the sixteenth century, and this district is mainly populated by negroid elements. Among the surviving Indian tribes some cultural traits of African origin can still be observed in their folk tales, woodcarvings, drums, etc.

As far as I am aware, no other tribe in Colombia, or in South America, uses pipes of this type.

Cartagena, Colombia GERARDO REICHEL-DOLMATOFF

**Kadan Religion.** Cf. MAN, 1955, 161

75 Sir.—The Revd. Father M. Hermanns, S.V.D., Bombay, arrives in his 'Contributions to the Study of Kadan Religion' at the conclusion 'that the High God of the Kadan belief is the original creator and genuine ruler of the world and of mankind.' He repeatedly quotes my monograph, *Kadar of Cochin*, Madras, 1952, in this connexion (pp. 14, 18, 154f., 161f., 163, 165ff., 173ff., 180, 183, 196f., etc.) and refers to reports of his own Kadan informants, who gave him various versions of mythology relating to Parameshva and Aiyappan. In those versions the former, Parama Shiva, was identified with Surya (Sanskrit for sun), whilst Ilura (moon) his 'sister' was identified with Parvathi, Shiva's

*Shakti* or 'wife.' In spite of this, Father Hermanns states: 'The female counterpart in this couple plays no special role and is hardly mentioned.' He furthermore considers these and similar versions of Shiva-worship as representative of what he calls an 'inner circle' of 'pre-Dravidians.'

Paramesha, a Sanskritic reference to Shiva and his *Shakti* Parvati, are the well-known deities prominently worshipped by the Hindus. Aiyappan or Sasta, the son of Shiva and Vishnu—the latter here appearing as a female—is also ardently worshipped by South Indian Hindu plains people of the west coast, especially at Sabarimalai in Travancore-Cochin where he is not identified with Malaiadivam. Malaiadivam, a supposedly 'pre-Dravidian deity, originally revered by the Aborigines' (L. K. Bala Ratnam, *Sasta Worship in South India*, Trivandrum (Sridhara Printing House), 1943, p. 29) has a separate Malai (Hill) set up near the famous Sasta shrine at Sabarimalai. Sasta and Aiyana images are at other shrines accompanied by two female figures, one on each side, which are identified as Purnamba and Pushkalamba, the consorts of Sasta (*ibid.*, p. 88).

Surya is a Sanskrit name for sun, here identified with Shiva, whilst Ilura (moon) is seen as his sister, instead of his wife. I have referred to numerous such religious acculturation features, but, for reasons given on pp. 167f. of my monograph, could not consider them to be part of the originally Kadar set of beliefs. But even granted that Shiva-Shakti-worship among Kadar was representative of Kadar religious sentiments, as Father Hermanns seems to suggest, this worship can by no stretch of imagination be considered less bound up with a female aspect of the divine principle than the Kadar concept of *Malavay* and *Malankurathi*, their own divine couple of hill deities.

In fact, Father Hermanns himself quotes his Kadar informants of the Ambula group as having stated that: 'Our main god is Parama Shiva, whose consort is Parvati,' and Kanakuthy as saying that 'Parama Shiva and his consort in heaven had a son.' These tales about Shiva and Parvati are in keeping with Hindu mythology of the plains in which the female aspect of the divine couple is also mentioned and plays a role which is not negligible.

Of the many inconsistencies which Father Hermanns's informants seem to have told him, and which he reproduces without comment, I would like to mention only two: first, Valakayan of Kuryarkutty stating that he was admonished 'to pray to one god only' by his mother who 'constantly mentioned Paramesha (Parama Shiva) and Aiyappan' (my italics); secondly, the identification of the double-pronged mountain Karimalai (Gopuram Kovil) with the divine couple Malavay and Malankurati, although these mountains were, according to Kadar belief, created by—not identical with—the divine couple and could not have fought, etc., until after their creation.

I have referred to the motivation for such statements made by some Kadar informants (1952, p. 194), where I describe the desire of the more sophisticated among them to appear in every respect, and therefore also in their religious worship, the same as the peoples of the plains. Since my fieldwork among the Kadar in 1946-47, this trend must have considerably increased—particularly under the influence of the new slogan: 'There is no caste—we are all Indians.' Hindu terminology and mythology may also have further spread. Shiva (and his *Shakti*, for that matter) or Aiyappan (Sastan) can, under these circumstances, hardly be considered as original Kadar concepts, in spite of the attempts made by some individual Kadar to have us believe that.

University of Madras

U. R. EHRENFELS

**76** Sir.—I have carefully gone through the article on Kadar Religion by the Revd. Father M. Hermanns of Bombay, recently published in MAN, and I write to give a geographical interpretation of the native beliefs. The 'great knowledge of traditions and customs' is the retention of old habits as crystallizations. Such crystallizations of past culture are direct response to the extent to which a people have lived apart. In other words, the preservation is a measure of the extent to which modern ideas have been excluded from the region. Roderick Pettie cites the example of how the 'suckling infant, now a man, has inherited from his grandfather old tales. He knows and sings the same themes and

sings them with the same ancient verbiage and inflection' (*Geography in Human Destiny*, Armed Services edition, pp. 6f.).

The worship of the Sun God (Surya) may be ascribed to the fact that for the tribes living in thick jungles the light and heat of the sun is vital. The concept of God and the consequent worship of any religious deity is closely correlated with the external environment, e.g. 'The Eskimo hell is a place of darkness, storm and intense cold, the Jews' is a place of eternal fire' (E. C. Semple, *Influence of Geographical Environment*, 1947, p. 41). The tribal stories are also woven around the objects known to them. The degree of their conquest over them confers on the environmental features concerned a superior position over other features; e.g. mountains for these people are the seats of God Aiyappan and Goddess Kaliyani while the forests are full of ghosts. The offerings reflect the availability of materials on the one hand and the limitation of products on the other. It also hints at the proper and extensive utilization of natural resources left untapped as yet, e.g. offerings of rice, coconut and plantations by the Cherpa group of Kadars. The 'female counterpart plays no social role' since, the environment being hostile and inhospitable, it is the menfolk who face it and females consequently occupy the secondary status in the social group.

In the Ambula group a section retreats to the 'poor and restricted districts becoming lesser in number.' The growth of numbers is related to the food supply; e.g. the rice zones are the areas of thickest density in the world. Scarcity of food, coupled with the difficulty of eking it out, is reflected among the Ambula in lack of numbers. Sometimes this fact leads to certain taboos and customs such as polyandry among the hillmen of Chakrata (Dehradun, U.P., India). Such a marriage custom means that there is only one woman to produce offspring; hence a limit has been deliberately set to the growth of numbers.

The 'Pujari gets possessed of the spirit.' Such and other superstitions have been elucidated by K. M. Panikkar in his *Hindu Society at the Cross Roads* (Asia Publishing House, Bombay, 1955) where he writes that certain groups, chiefly the priestly class, ascribing a superior status to birth in the upper castes, stuck to certain practices (as in this example) to maintain some distinction in order to keep up their superiority over others. Saying that the priest gets possessed of the spirit is a superstition and may be called a mere psychological or emotional satisfaction on the part of the innocent tribal people. Very probably the priest deliberately pretends to be so for his own purpose.

In the Valakayan Group stone-worship exists and the blood of a cock is sprinkled to please the deity. Among the tribes stone-worship symbolizes in one way or another the Creator, Preserver and Destroyer, all in one, an object of horror—hence the awful reverence accorded to it. The devotee sacrifices whatever is best according to his own judgement: cock, to his mind, is the best meat (as to many of us too) which he can offer to his God. The concept that the body is of the earth and goes to it while 'God has given [the soul] and he takes it back' is a noble one. It is fascinating on the one hand and surprising on the other that Valakayans are possessed of such an ethereal concept of God. Further, there are no temples, but stones are put under the trees. The absence of temples may be due to lack of skill on the part of natives in utilizing the building material at hand, or this might have been so in the beginning, while now it is by the force of tradition that they do not cover stones. In the practice of putting the stones under the trees is reflected their sense of reverence for Gods whom they wish to protect from rain and sun by placing them in the shade. Further, spirits are supposed to dwell in trees because the trees are serving their Gods by covering them and also by their virtue of being in close proximity to them.

Before I conclude I must say that some points are not clear in this article. Father Hermanns writes that 'the shifting cultivators belong to a pre- and non-Dravidian population.' What was this population and whence did they come? Further he writes: 'Pujas are performed and offerings of rice boiled in milk, sugar . . . are made to Him,' while nothing has been said of the milch cattle and pasturing. Some more details regarding the impact of religion upon the social life would have been very much appreciated.

Department of Geography, Nagpur University,

S. D. MISRA

Nagpur, Madhya Pradesh, India

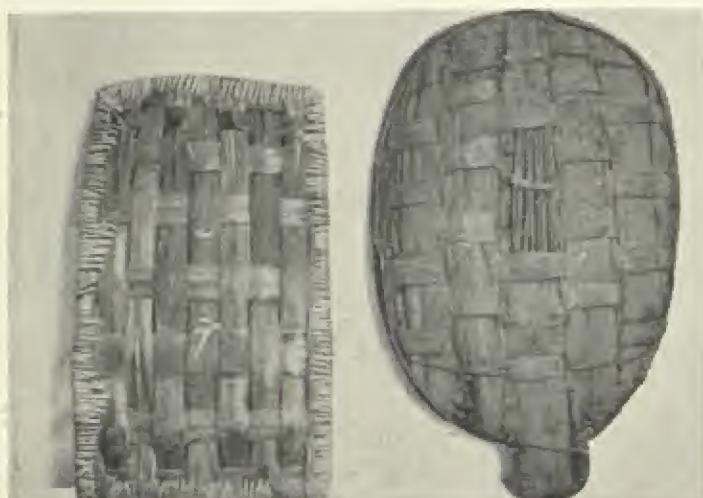




(a) War helmets to protect the head against blows, Ibo



(b) Two twine war helmets, one bark helmet in imitation of a soldier's fez and a priest's cap, Ibo



(c) Two war shields made of the midribs of the oil palm, Ibo



(d) Miniature, magical war shields, Ibibio



(e) A group of Ibo warriors in ancient battle dress



(f) A funeral wake among the Ekoi. One warrior wears a string helmet and holds a fighting stave.

# IBO WARFARE\*

by

M. D. W. JEFFREYS, M.A., PH.D.

*University of the Witwatersrand, Johannesburg*

77 Very little is known about the manner in which the Ibo conducted warfare in the days before the European arrived. Basden points out that in the early days of his missionary work:

During the dry season fighting was a sort of pastime, either between different quarters of the same town or between neighbouring towns. In the former case, i.e. civil war, firearms were not always used, but recourse was had to knives and staves. Helmets woven from the dried stems of the largest koko leaves (*akasi oyibo*, *Colocasia angolensis*) were worn which gave fair protection to the head but left the face exposed. Some men wore doublets made of the same material, others had chest and back protectors fastened over the shoulders. The majority carried shields composed of plaited palm stems (*igu*). The knives were the ordinary trade matchets, and these, when well sharpened on a stone, and in the hands of a hefty warrior, were dangerous weapons. On the days of battle the noise and excitement made up for that which was lacking in blows, and frequently preliminaries were prolonged beyond all apparent necessity. As a rule fighting ceased for the day when a warrior was killed; the parties straightway draw off. The one side to rejoice and make merry with feasting, the other to mourn the loss of a comrade.<sup>1</sup>

In more recent days the same sort of thing is obvious in Ibo fighting, as Basden observes:

Battles are fought in a haphazard manner; there is little attempt at leadership or organization. Each unit follows its own devices, advancing, firing or turning tail exactly as it feels inclined. Those whom one would naturally expect to lead soldiers in battle, i.e. the chiefs, are never present. It is convenient for them to retire to a remote spot in the bush, well away from the fray, where they can in quietness (and security) invoke the help of the gods. When there is no further need to pray they return home.<sup>2</sup>

Further light on what ancient Ibo fighting was like may be gained by realizing that the Ibo never used the bow and arrow either in war or the chase. The bow and arrow, degenerate miniatures of something better, are used by small boys in games of skill or in hunting rats and birds. What then did the Ibo use before the European arrived?

One's mind turns inevitably to spears and swords and of course the Ibo ironworkers from Awka are, according to Basden, '...renowned throughout the Ibo country and even beyond its borders, as clever blacksmiths, and they traverse the country from end to end plying their craft.'<sup>3</sup> These men turn out excellent matchets or pangas and good spears, but it is to be noted that these men are blacksmiths, not foundrymen. There is, roughly speaking, no iron ore in Ibo land and no sites where iron was mined, and there are no Ibo foundrymen. Consequently the Awka blacksmiths have had to rely on purchased or imported iron for their craft. Before the European arrived iron was a rarity among the Ibo—so rare that even in 1930, when I was at work among the Ibo around Awka, the centre of their iron industry, I found Ibo women, like their sisters the American Indian<sup>4</sup> women among whom iron was unknown before

the arrival of the European, using the shoulder blades of animals as hoes. Iron was even rarer among the contiguous Ibibio who until fairly recently were using wooden shovels in the farms and wooden mallets as illustrated in fig. 1. These articles are from Ikot Ekpene and were collected in 1928.

Consequently it is not surprising to find that in the early days iron seldom figured in the armoury of an Ibo warrior. He was satisfied to fight with wooden swords and spears.



FIG. 1. WOODEN SPADES AND MALLET, IBIBIO  
The longest is about three feet long

Mary Kingsley, writing in 1897, remarked of the Ibo: 'The spears of this interesting people are even to this day made entirely of wood.'<sup>5</sup> A photograph with which I had intended to illustrate these weapons, but which has unfortunately been mislaid, shows two wooden fighting sticks or thrusting (not throwing) spears and five wooden swords which look rather like canoe paddles. As the nearest river was the Niger, some 30 miles away, any suggestion that their origin or use was as canoe paddles may safely be excluded. The first of the items shown in the lost photograph is a fighting stick, *nkpo*, 65 inches long, used in the early days before the Europeans arrived. A blow or a thrust with this stave was reckoned to kill a man instantly. Because some warriors are protected by charms against bullets, this stick is used to attack and kill such men. Next is a wooden sword with a leaf-shaped blade, curved, wooden hilt terminating in a knob, 43 inches long and 3½ inches broad. These wooden swords are called *abariba*, after the people of the same name who, in the old days,

\* With Plate F and two text figures

were not killed by bullets but by these wooden swords. These swords were used in fighting the Abam and Amuchukwa as well as the Abariba. The reason that bullets were ineffective against these slave-raiders was believed to be the charms worn to protect their bodies; actually their immunity to bullets was due to the high degree to which gunpowder sold to the Ibo had been adulterated with charcoal. The next item is not a spear or fighting stick but is a sword with a square cross-section, 41 inches long and 1½ inches wide. The next two are ordinary wooden swords, the last one is of wood from the *ugulo* tree. It has a leaf-shaped blade with a rib running down the centre, and the hilt terminates in a knob. The implement is 28 inches long and three inches wide. The *ugulo* is the *Garcinia kola* and according to Dalziel: 'The wood is yellow with a brownish centre, hard, close-grained, finishing smoothly and taking a good polish . . .'<sup>6</sup> He makes no mention of it being used for wooden fighting swords.

As a defence against these and other weapons the Ibo had devised fibre 'crash' helmets or *okpu agha*. Those shown in Plate Fa are entirely plaited out of the coarse fibre in the stems of *Colocasia antiquorum*. Dalziel remarks: 'The Ibos use caps or helmets and a kind of armour woven from the fibre got from the petioles.'<sup>7</sup>

The headgear shown in Plate Fb consists of two war helmets knitted out of twine made from the fibre of a hibiscus plant, and one priest's cap, also made from a hibiscus plant but from one different from that used in making the war helmets. This fibre fez is indicative of the rank of the priest who wears it. The remaining helmet is in imitation of the fez worn by the African soldier in the Nigeria Regiment. This helmet is quite serviceable because it is made out of the stout bark of a tree and would effectively protect the skull from a blow on the head.

The Ibo warrior also carried shields which were of two types. One was a heavy wooden shield. This was used for home defence, when defending a town against attack. It was too heavy to be carried on raids or forays and was then replaced by a light wicker shield made from laths cut from the midrib of the oil palm (*Elaeis guineensis*) or of the Borassus palm (*Borassus aethiopica*). These wicker shields are found widely distributed. The Ogoni in Calabar province use them. Meek has something to say about these shields which is of interest. He writes<sup>8</sup>:

In contrast to the small round leather shield (used by bowmen and believed to come from Asia) we find also the small round shield made of woven strips of palm bark. This type belongs to what Dr. Montandon calls the pre-Malayo-Nigritic group, and is found among the Berom and Ganawuri of the Bauchi Plateau, and the Kagoro of Nasarawa Province. The ancient Jukun are said to have used round wooden shields. The Berom shield is about 20 inches in diameter, and the material used is the bark of the fan-palm . . .

As palms do not have a bark, the references to the bark of a palm must be read as referring to the midrib of a palm frond. The illustration No. 85 in Meek's *Northern Nigeria* shows a wicker shield identical with that made by the Ibo from the midrib of the frond of an oil palm. Two such shields are shown in Plate Fc.

In addition, the Ibo warriors often wore round their

bodies a stomacher of woven fibre. Fig. 2 shows two Ibo warriors making the best of two worlds. Both wear their 'crash' helmets while one wears a stomacher of woven fibre. Each carries an iron knife made by an Awka blacksmith, each is armed with a flintlock made in Europe. The powder pan on each gun is protected from the rain by a funnel of goatskin that is pushed up along the barrel when the gun is to be fired.



FIG. 2. TWO IBO WARRIORS COMBINING ANCIENT AND MODERN WAR EQUIPMENT

Plate Fe is a photograph taken of a group of Ibo warriors who had turned out in their traditional war dress to honour the memory of a fellow warrior at his funeral. In this photograph, probably the last of its kind, can be seen wooden spears, wooden swords, fibre helmets, fibre shields and two fibre stomachers.

This type of war accoutrement was common to all the rain-forest tribes but I was unable to obtain any among the Ibibio. All that remained to them were the miniature shields used by their Ekong or war society when honouring the obsequies of one of their members. Plate Fd illustrates these shields. With the exception of the centre shield called *iba*, the other four afford only magical protection in war. This *iba* is sufficient to protect the user against spears and weakens the power of bullets. The top, small shield, on the left, is called *utite ogo* or war medicine. Its protection is magical. The shield below it is called *ekpeke* and measures

8½ inches by 6¾ inches; its protection is also magical. If it is held before one, bullets cease to have any effect. Eggs are sacrificed over it each month to maintain its magical potency. The names of the two shields nearest to the right are *ikpo ngba* for the top one and *ekpeke* for the bottom one. The *ikpo ngba* is worn at the waist by wrestlers. It is also worn at funeral obsequies.

This type of war equipment I have found among the Ekoi who live in the forests on the left bank of the Cross River. In Plate Ff a man wearing a string woven helmet with a wooden sword in his hand can be seen at a funeral celebration.

According to Aro tradition it appears that when they engaged Ekoi mercenaries from Ibom to attack the Ibibio and drive them from their shrine of Ibritam (later known to Europeans as the Long Juju), the Aro imported certain Ibo from Amaseri who were skilled in making 'war caps.' Amaseri is about four miles south-west of Afikpo. Mr. H. F. Mathews in an unpublished Report on the Aro, written in 1927, says: 'Amaseri is renowned to this day for the manufacture of caps which appear to be of knitted or crocheted coarse string, quite a useful substitute for a leather helmet probably well padded with a variety of amulets.'<sup>9</sup>

It thus becomes apparent that fighting among the Ibo in the old days partook of the nature of individual tourneys or combats and that with the wooden weapons and protective armour it was rare for a man to be killed or even seriously injured. In fact, war was a friendly but exciting display of human force employed to break up the monotony of the dry season.

Most of the material described in this article is now in the Pitt Rivers Museum, Oxford, thanks to the munificence of the Wellcome Foundation in providing me with grants to make the necessary purchases.

#### Notes

- <sup>1</sup> G. T. Basden, *Among the Ibos of Nigeria*, London, 1921, p. 202.
- <sup>2</sup> *Ibid.*, p. 204.
- <sup>3</sup> *Ibid.*, p. 78.
- <sup>4</sup> R. H. Lowie, *An Introduction to Cultural Anthropology*, New York, 1946, p. 28.
- <sup>5</sup> Mary Kingsley, *Travels in West Africa*, London, 1897, p. 58.
- <sup>6</sup> J. M. Dalziel, *The Useful Plants of Tropical West Africa*, London, 1937, p. 91.
- <sup>7</sup> *Ibid.*, p. 482.
- <sup>8</sup> C. K. Meek, *Northern Nigeria*, Oxford, 1925, Vol. I, p. 311.
- <sup>9</sup> H. F. Mathews, Report on the Aro as Anthropological Officer, Southern Provinces, 1927. Unpublished Government MS.

## EROTIC IBIBIO TONE RIDDLES

by

DONALD C. SIMMONS

*Stamford, Connecticut*

**78** The Ibibio inhabit several districts in Calabar Province, Nigeria,<sup>1</sup> number approximately 800,000 to 1,000,000,<sup>2</sup> and speak a language in which tonality constitutes an important element of semiosis. A special form of riddle exists in Ibibio folklore and consists of two parts, a question and an answer, both possessing identical or very similar tone patterns although differing in meaning. I denote this form of folklore, which also occurs among the neighbouring Efik,<sup>3</sup> a 'tone riddle.'

Example 1 evidences the one-to-one correspondence of the question-and-answer tones. In this and the following examples syllabic tones are notated as follows: low tone no notation, high tone 1, mid tone 2 (this includes both mid tone and a tone higher than mid but lower than high which occurs with high-toned verb roots), falling tone 3, and rising tone 4. Consonants and vowels are pronounced similarly to their English equivalents except that *h* is a velar fricative, *kp* is a labio-velar consonant, *ng* represents the sound of the middle consonant in English 'singer,' and *o* includes the vowel sound in English 'law.'<sup>4</sup>

(1) Q. <i>asak</i>	1 1	2	1 1
species of snake	<i>fro</i>	<i>usung</i>	
R. <i>itit</i>	1 1	2	1 1
vagina	<i>dioho</i>	<i>okpong</i>	
	settles	fat.	

Example 2 evidences a slight difference between tones of the question and answer. In comparing tones of examples

it may be helpful to know that falling and rising tones can be interpreted respectively as a change from high to low tone, or from low to high tone. Similarly, mid tones are interpretable in some instances as a change in tone position from low to high tone, or from high to low tone, depending on whether the preceding syllable possessed low or high tone. Vowel elision may also alter the tone of a syllable and occurs, depending on speed of utterance, when a morpheme terminating in a vowel precedes a morpheme commencing with a vowel.

(2) Q. *ukana* using *idim*  
oil bean tree of road of spring.

R. *efa* <sup>3</sup> *nyin itit*  
comb of clitoris (*i.e.* the tip of the clitoris).

Among the Ibibio the tone riddle serves as a form of amusement, greeting, explanation for an action, indirect method of cursing, and erotic *double entendre*. Although it is this last function that these particular examples illustrate, the other functions may be mentioned briefly. When two friends meet they sometimes use tone riddles to replace the prosaic customary greetings; one says the question of a tone riddle and the other replies with the correct answer. Since cursing frequently results in a suit for slander before the Native Court the tone riddle may be used to express disregard and contempt for an enemy without actually cursing him; a person simply says the query of a tone

riddle that possesses an answer which denotes a derogatory meaning and thus obviates any accusation of cursing since the query may be construed as a direct utterance rather than an indirect curse. Occasionally the tone riddle serves as a periphrastic method of explanation; if asked the reason for doing some particular action an individual may reply with the query of a tone riddle and thus intimate the reason since the answer to the riddle supplies the explanation.

The tone riddle readily lends itself to the formation of erotic answers to ostensibly innocent statements. In example 21 the innocuous phrase 'trunk of palm tree falls and blocks the road' is interpreted to mean 'clitoris goes out and wants food.' Men cite erotic tone riddles for amusement, and to embarrass or insult women. If several individuals pose tone riddles to each other a man may cite the query of an erotic tone riddle and request, to the amusement of his audience, that a woman answer.

All examples contain some reference to the vagina, clitoris, or coition. No examples refer to *fellatio*, bestiality, or pederasty since these are known only from stories told by Ibibio who served in Europe during the Second World War, or by rumours of the activities of an occasional European pervert living in Southern Nigeria. Although example 27 alludes to cunnilingualism the allusion serves only to suggest a concept so impossible for an Ibibio as to be ludicrous. Several examples refer to such anatomical facts as that the external female genitalia may be adipose (1), be red like the fruit of the wine-palm tree (30), lack hair on the clitoris (25), and be odoriferous because of its proximity to the anus (6). Examples 22 and 23 refer to the vaginal odour, and these specific tone riddles frequently function as indirect curses since the imputation of possessing an odoriferous vagina is one of the worst possible curses for an Ibibio woman. Examples 29 and 30 utilize English numbers as pronounced by Ibibio-speakers in the formation of the question. Example 18 is a parody on the numerous Ibibio proverbs which emphasize the necessity of family solidarity by delineation of the sad fate of an *ikpong*, 'lonely person' or 'person without a family.'

The distribution of tone riddles in Africa is unknown since students of African folklore all too frequently concentrate on the Uncle Remus type of folktale, proverbs and songs while overlooking such forms of folklore as tongue-twisters, puns, stereotyped sarcasms, nicknames, curses, ballads, tone riddles, and historical or biographical commemorations. I found tone riddles extant in the three Efik-Ibibio dialects investigated, namely Oron, Ibibio, and Efik, and believe that tone riddles will probably be found widely distributed among African groups south of the Sahara. The folklore of any group speaking a language in which pitch is lexically significant and contrastive should be carefully investigated to determine the possible presence of folklore forms based on tonality.

- (3) Q. *akpan* *ekpo udo* *ama* *idem ikpong*  
eldest son of Ekpo Udo loves self alone.  
R. *nyin itit* *itiata* *okpoho* *iba*  
clitoris eight (costs) manillas two.

A tone riddle said by russians during a famine to insult women meaning the cost of intercourse is very cheap since one can have eight women for sixpence.

- (4) Q. *afia usimg* *ete* *ikpisaha* *ita*  
main road says he would branch in three  
*kikpisaha* *inang* *keukot etim* *oboho*  
he would branch in four that leg pounds him too much.
- R. *itit* *etc* *ikpinyie* *okuk* *3 i* *kikpifak* *ita*  
vagina says if it has money it would initiate buffet  
*ke* *mfet* *ekung* *oboho*  
that penis has intercourse with her too much.
- (5) Q. *ekpat* *oyep* *1 2* *odo* *ngkamba*  
trunk of oil-palm tree falls both sides.
- R. *nyin itit* *odiongo* *uko iyak*  
clitoris knows fishing.
- All non-Ibo tribes of the upper Cross River area are called 'Atam' by the Ibibio and Efik. Many 'Atam' women in Calabar fish in the daytime and harlot at night.
- (6) Q. *ekpo* *ikot* *okpo mbo* *owot* *ekpo ikot* *udo ekpo*  
ghost of Ikot Okpo Mbo kills ghost of Ikot Udo Ekpo.
- R. *ngkanga* *ufik udwang* *esit nto* *owot nyin itit*  
dry smell of excrement inside latrine burns clitoris  
*ekiso*  
on forehead.
- (7) Q. *idiang* *okpo usimg* *isioho* *ntan*  
cricket of main road doesn't take out sand.
- R. *orok* *nyin itit* *ifata* *owo*  
no-nose of clitoris doesn't bite person.
- 'No-nose' is a disease which attacks the nasal mucous membranes and results in the eventual extirpation of the nose. The tone riddle alludes to the similarity in shape of the nasal orifice of a 'no-nose' sufferer and the vagina.
- (8) Q. *iko esa* *akpan*  
cup of yard of eldest son.
- R. *awak* *ngkwa itit*  
you have many eggs of vagina.
- (9) Q. *ikpo nsung ikang*  
big ships.
- R. *okpon ckpong itit*  
big clitoris.
- (10) Q. *ikang ata* *ikot owo* *idim ebet* *mbere nsukakara*  
fire chops bush stops at boundary species of tree  
*milo* *ukpi*  
if you grow I cut you.

- R. *mkpak oduk itit*      I 4      I      I  
penis enters vagina      stops inside      beats      drum  
I I I      I 2      I 2  
nyin itit adaha      emik  
clitoris departs (and)      dances.
- (11) Q. *ikpo oyo eben*      1 3      I      I 2  
big bunch of African pears      ndon udo otung  
R. *ikpo mkpok itit mmbo*      3      1 1      1 2  
big skin of vagina receives my semen (and) throws  
away.
- (12) Q. *iva oduk ofro okpo usung*  
cassava      enters      jumps      main road.  
R. *mari okopon okobo nyin itit*  
Mary is fat (and) has curved clitoris.
- (13) Q. *mininyong urua akpa enang*  
plantain of market costs (a) cow.  
R. *okop inem itit awak aya*  
you feel sweetness of vagina (and) tear sleeping mat.
- (14) Q. *ndok obot urua intyang ngkekit asabo kuruan*  
I climb hill of Urua Inyang (and) go see python in Uruan.
- R. *ngkung eyin akan angwan ngkesio*  
I have coition with child of old woman (and) go take out  
I I  
nsema kitit  
beans from vagina.
- (15) Q. *ntabit ntok, ntok*  
species of plant very small.  
R. *ingwot usung esit itit*  
show me (the) path of inside of vagina.
- (16) Q. *ntabit ntuck ntuck*  
species of plant (has) small bunches.  
R. *mkpak odo okok itit*  
penis is chew-stick of vagina.
- (17) Q. *ntorodo sak okom mya udieok*  
food dish burnt you think I will throw you away  
I I  
ntekpang  
like fufu cake.
- R. *ndubene site okom mya ukung*  
wall bore hole you think I will have coition with you  
I I  
ntitit  
like vagina.
- (18) Q. *ngkeng odioho okpo usung*  
large basket stays on main road.
- R. *ikpong lonely person*      I      2      I  
angwanga has broad odu hole of vagina.  
(19) Q. *ngkpo ikot*      I I I      1  
thing of bush (i.e. termite)      idoho      popo  
doesn't climb pawpaw tree.
- R. *nyin itit idiaha fufu*  
clitoris doesn't chop fufu.
- (20) Q. *ngkpo ikot*      I I I      1 1  
termite      idoho      osokoro odung  
doesn't climb Seville orange tree.
- R. *nyin itit idiaha ewesara enim*  
clitoris      doesn't eat sweet species of yam.  
The ewesara enim yam is phallic-shaped.
- (21) Q. *okon oyop*      1 2      1 2      2 2  
trunk of oil palm falls (and) blocks road.
- R. *nyin itit owo eyem udia*  
clitoris goes out (and) wants food (i.e. penis).
- (22) Q. *oko ebok ekpekkpen*      1 2      1 3      1  
he-goat if big doesn't climb cow of iron (i.e. bicycle).
- R. *ovo ngwan ekpekkpen inanake afik itit*  
person female if big doesn't lack smell of vagina.
- (23) Q. *ofiong ada kata onyong*  
moon stands on real sky.
- R. *itit otongo kc neck etebe*  
vagina begins from fresh smell.
- (24) Q. *okono ofro usung ami mkipi ekobo ikwa mimi*  
rat jumps road I cut with bent machete my.
- R. *abaikpa eyim itit ami ngkung*  
girl refuses vagina I have intercourse (and)  
3 2  
embre ndidet  
play with her on sand.
- (25) Q. *okpo usung ifite*      1 1 1      4 3 2  
main road doesn't grow species of grass.
- R. *nyin itit ifite mfuk ekot*  
clitoris doesn't grow hair on occiput.
- (26) Q. *udo etok okpo enek unek ikpat isime*  
Udo Etok Okpo dances dance foot doesn't reach  
I I      I 1 2      1 1      1 3 2  
isong idata ntan  
ground (and) touch sand.
- R. *itit etok eyin adia ntan eka*  
vagina of small child eats sand mother  
I I      1 1 1 1  
ifefit ikapakap  
blow out (with stick).

(27) Q. *ukom* *ufap* *anfiono* *ufap*  
plantain I climb you you are slippery to me I climb you  
*anfiono* *obo* *udok*  
you are slippery to me do you want me to climb you (with)  
*ikpo*  
climbing rope?

R. *itit* *ukung* *enneme*  
vagina I have intercourse with you you be pleasant to me  
*ukung* *enneme*  
I have intercourse with you be pleasant to me  
*obo* *udia* *kinua*  
do you want me to eat you with mouth?

(28) Q. *utin* *asaha* *edito* *isong* *asiak*  
sun shines (and) come hits ground (and) splits.  
*itit* *okupo* *ediben* *nsung* *ata*  
vagina opens (and) come takes fly (and) chews.

(29) Q. *tri* *naintin* *twenti* *tri*  
three fifteen twenty three.  
*itit* *obo* *nsung* *nyong* *ekikoi*  
vagina receives errand home (to) hips.  
(30) Q. *wun* *nainti* *tri*  
one ninety three.

R. *itit* *adat* *etube*  
vagina is red (like) fruit of the wine-palm tree.

## Notes

<sup>1</sup> I thank the Church of Scotland Mission and its missionaries for many kindnesses, the Department of Anthropology, Yale University, for a grant which aided my research, and the Government of Nigeria for permission to conduct ethnological research in Calabar Province during 1952-53.

<sup>2</sup> D. Forde and G. I. Jones, *The Ibo and Ibibio-speaking Peoples of South-Eastern Nigeria*, O.U.P., 1950, pp. 67f.

<sup>3</sup> D. C. Simmons, 'Specimens of Efik Folklore,' *Folklore*, Vol. LXVI, 1955, p. 423; see also 'Cultural Functions of the Efik Tone Riddle,' forthcoming in *J. Amer. Folklore*.

<sup>4</sup> The author's orthography has been slightly simplified in these two last respects.—ED.

## OBITUARY

Frederic Wood Jones, F.R.S.: 1879-1954. *With a portrait*

**79** The announcement of the death on Wednesday, 29 September, 1954, of Frederic Wood Jones brought feelings of infinite regret and great personal loss to thousands of his friends, students and fellow scientists all over the world. To many it must have come as a great surprise and shock, for he still appeared to be, despite his 75 years, the energetic, alert-minded and invariably cheerful companion that we had known for so long. But, to those closest to him, it was apparent that for almost two years he had faced with patient endurance and rare courage a steady decline in health.

Of him it can truthfully be said that no one can take his place. The medical man whose interests range widely through the field of comparative anatomy, anthropology and biology is no longer easy to find and there is no doubt that Professor Wood Jones held a unique position in that he was outstanding in all these branches of learning and was no mean authority in many others. In one of his lectures delivered in Adelaide in 1937 he spoke of the decline of that 'Spirit of Adventure' that in former days inspired the student, and particularly the medical student, in his pursuit of knowledge and persisted into his professional life. In these days, however, there are but a few 'who realize that a medical qualification will enable them to live a life of adventure.' Such men, he says, are not necessarily 'dreaming of the wild places of the earth, but they have ideals of some more romantic setting for their lives than the fashionable medical quarter of their home town.' How well in his own life he had preserved this 'spirit of adventure,' this perpetual urge to find the true facts, be it of a medical problem or an inquiry into any of the unsolved questions in that wide field of natural history that he found so absorbing. The satisfaction of finding the answer was to him far greater than the mere acquisition of worldly goods and honours.

Frederic Wood Jones was born in Shadwell, London, on 23 January, 1879. Like John Hunter, whose ideals he so greatly revered throughout his life, he spent all the time he could spare

from his school work in exploring the countryside, observing and recording natural phenomena. After the usual preliminary education, part of which was gained at the Enfield Grammar School, he entered the Medical School of the London Hospital. In a letter written in 1946 to Sir Arthur Keith he remarks: 'It was in 1897 I resolved I would try to be what you were—an anatomist, loving the subject and all its ramifications.' In 1900 he wrote his first scientific paper: 'A Note on an Outgrowth of a Lymphoid Nature from the Junction of the Large and Small Intestine of a Frog (*Rana temporaria*)', and this was to be followed during the next 54 years by more than 300 publications on a wide range of subjects. In 1903 he gained the degree of B.Sc. of London University and in the following year graduated M.B. He spent the following two years as medical officer in the Cocos-Keeling Islands and there met his future wife, the daughter of the governor, George Clunies Ross. In 1907 he joined Professor Elliot Smith in Egypt to assist in making a study of the anthropological material that was made available when, by the decision of the Egyptian Government, an archaeological survey, directed by Dr. G. A. Reisner, was undertaken of that part of Nubia that would be submerged when the level of the Aswan Reservoir was raised. The Bulletins containing his findings were published by the Survey Department of the Egyptian Ministry of Finance in the following year. On completing this investigation and at the request of Professor Elliot Smith, he went to Manchester as Lecturer in Anatomy, but in 1910 he returned to London and served as demonstrator both at the London Hospital and at St. Thomas's Hospital Medical Schools. At the outbreak of hostilities in 1914, he made strenuous but at first unsuccessful efforts to join the R.A.M.C. and when finally accepted served at the Military Orthopaedic Hospital at Shepherd's Bush, directed by Sir Robert Jones. In 1919 he was invited to take the Chair of Anatomy in the University of Adelaide where he remained for seven years, during which time, in 1925, he was elected a Fellow of the Royal Society. In 1927 he accepted the post of Rockefeller

Professor of Physical Anthropology in the University of Hawaii in Honolulu and during the tenure of this office he wrote, in conjunction with Stanley D. Porteus, Professor of Clinical Psychology,



FREDERIC WOOD JONES

*The Matrix of the Mind*, published in 1929. In 1930 he was appointed Professor of Anatomy in the University of Melbourne, which post he held for seven years, with one short break in 1932

when he left Australia to act as temporary Director of Anatomy at Peiping Union Medical College. On 8 February, 1938, Professor Wood Jones arrived in England, having accepted the Chair of Anatomy in the University of Manchester, left vacant when its previous occupant, Professor J. S. B. Stopford, was appointed Vice-Chancellor. For the next eight years he worked strenuously at his researches, gave more than the usual number of lectures to students, owing to shortage of staff consequent on the urgent demands of the War Office, and took his share of the many extra duties that all were called upon to perform at this period.

In 1941, he learned with the greatest distress of the bombing of the Hunterian Museum of the Royal College of Surgeons and at the request of the President, Sir Hugh Lett, drew up a memorandum containing suggestions for its reconstitution when that should be possible. In 1945, through the generosity of Sir William Collins, funds were made available for the establishment of a Chair of Human and Comparative Anatomy at the College and it gave him the utmost satisfaction to receive from Sir Alfred Webb-Johnson the invitation to become its first occupant. It is certain that nothing gave him greater pleasure than to be of such material assistance in building up again this unique collection and it must always be regretted that he did not live long enough to see it set out again as he would have wished in the new building now in course of construction in Lincoln's Inn Fields.

An appreciation of Professor Wood Jones would be sadly deficient if it did not contain some reference to the magic of his words, both spoken and written. As a lecturer he held his audiences spellbound. As a writer he had that touch of genius that made his subject, be it scholastic or recreational, enchanting to the reader. But perhaps the thing that endeared this great and good man more than anything to those who knew him was his unfailing kindness and patience. He was never too busy or too tired to listen to the difficulties of those who were eager to learn and he enjoyed no greater pleasure than in accompanying the enthusiastic research worker along the way to solving a problem. But he never failed to impress upon others the importance of accuracy and precision in scientific investigation and of him, as of John Hunter, it may well be said: 'He sought for truth, for truth's sake alone.' Many of his friends and students must look back to realize that the advice and practical help that he gave so unstintingly has set them on the sure road to success and perhaps his best memorial lies in imparting to others his ideas and ideals and keeping his example before us in all our undertakings.

JESSIE DOBSON

## Acorn-Leaching Basins: A Case of Convergent Development. By Robert F. G. Spier, Ph.D., University of Missouri

80

During the course of an ethnographical field study of the Chukchansi Yokuts Indians in the summers of 1949 and 1950,<sup>1</sup> a variant of the locally standard form of acorn-leaching basin was observed. Nothing much was thought of it at the time, but subsequent investigation revealed that the type is paralleled in only one other native Californian culture. While unfortunately all the data which one might wish for are not at hand, the case does bear consideration as a probable illustration of simple convergence.

Among the native tribes of California the acorn was a staple item of diet. It was most often eaten, after mealng, in the form of a gruel. Occasionally the meal was made into unleavened loaves or the acorns were cooked after only a preparatory shelling. In order that this seed may be palatable, the tannic acid which it naturally contains must be reduced in amount or entirely removed;

the acid content of acorns varies with the variety. Removal of the bitter radical was usually accomplished by leaching. One leaching method, infrequently employed, involved burying the acorns in mud for some weeks before shelling and cooking them. This technique, reported for the Shasta and Pomo, was evidently of rather restricted distribution, though some comparable soaking methods are reported from elsewhere. The commonest method of leaching was to pour water, either warm or cold, over the acorn meal.

During the leaching process the acorn meal must be held in a container which will confine it and at the same time allow drainage of the washing water. The native Californian practice runs in two general directions. Among the northern tribes the meal was contained in a sand basin, frequently a hollow scooped out of a sand bar along some stream. The southern Californians used an openwork basket to hold the meal. Some central tribes placed fir branches on the sand and spread the meal over these. All three of

these forms of leaching basin were employed by some central peoples, e.g. the Miwok. The modal Chukchansi Yokuts leaching basin was of sand, often, in the absence of sandy soils, spread evenly in an artificial depression in the ground. Yokuts practice generally followed this vein. The Northfork division of the Western Mono, eastern neighbours of the Chukchansi Yokuts, used the simple sand basin.

The Shasta methods of acorn-preparation were substantially those of the Maidu except for the leaching basin. It is described as follows: 'The woman prepared a small scaffold or platform of sticks (some 15–30 centimetres above the ground) resting on either forked sticks or on stones. On this a layer of pine-needles was placed, followed by a layer of sand, made thicker at the edges to form a basin. The meal, in a layer about five centimetres thick, was spread over this, and warm water poured on, as in the region of Central California.'<sup>2</sup> The Shasta also leached acorns by burying them in mud.

The variant Chukchansi Yokuts leaching basin observed was virtually identical with the Shasta type. The platform of sticks, resting on forked sticks, was somewhat higher, being about 30 inches (75–80 centimetres) above the ground, and similarly carried a sand basin. There can be no question of the typological equivalence of the variant Chukchansi Yokuts basin and the standard Shasta basin.

There exists no strong evidence to support a contention that this Shasta type of leaching basin was diffused to the Chukchansi Yokuts. The territories of the two peoples lie approximately

300 miles apart and travel along the Sierra Nevada foothills would have been difficult and unlikely. The Indian reservation situation in California was not one likely to produce the requisite contacts. The woman at whose dwelling the Chukchansi Yokuts example was observed had travelled to Sacramento on at least one occasion to conduct business in the Indian Service office there, but it seems most unlikely that leaching basins would ever have been the topic of conversation between Indians gathered in the office. She had evidently not travelled much under other circumstances.

One is led, therefore, to the conclusion that the parallelism between the leaching basins of the Chukchansi Yokuts and the Shasta is probably the result of independent development. It is possible that one of the types of Miwok leaching basins, the one using a sand layer on fir boughs placed on the ground, served as stimulus for the development of the elevated type among the Chukchansi Yokuts. It does represent a transitional form between the simple sand basin and the elevated type.

The above case of probable parallel development may serve as a caution to casual assumption of diffusion. We must assure ourselves that the cultural element in question could not have been the outcome of simple modification of previously existing forms.

#### Notes

<sup>1</sup> Sponsored by the Peabody Museum of Harvard University and partly supported by the sometime Viking Fund, Inc.

<sup>2</sup> Roland B. Dixon, 'The Shasta,' *Bull. Amer. Mus. Nat. Hist.*, Vol. XVII, 1907, p. 425.

## REVIEWS

### AFRICA

**Préhistoire de l'Afrique: Vol. I. Maghreb.** By Raymond Vaufrey. *Publ. Hautes Études de Tunis, Vol. IV.* Paris (Masson), 1955.

81 Pp. 458, 60 plates, 216 text figs. Price 3000 francs.

The first volume of Professor Vaufrey's comprehensive review of African Stone Age prehistory embraces French North-West Africa—Morocco, Algeria, Tunisia and the northern fringes of the French Sahara—and forms the introduction to his projected continental survey which will appear before long in a second volume.

It sets a fine standard for its own successor by reason of the detailed treatment of each culture, backed by a critical exposition of the associated geological and environmental evidence. It is beautifully illustrated by the master hand of M. le Chanoine Bouyssonie, who contributes some 3000 drawings of the various blade industries, some of which have already appeared in *l'Anthropologie* and elsewhere. Good drawings of the biface and flake cultures are by M. Bordes and others. The topographical and geological plates, of reasonable size and definition, are informative. Distribution maps and well placed text figures of field sections clarify a text packed with factual information and comment. Voluminous footnotes, tabular inventories and quotations in smaller type impair the symmetry of the page, but provide the *appareatus criticus* required of modern prehistory, which has been sadly lacking in so many French colonial and other publications.

Despite the careful work of a few individuals, the notorious defects of so much fieldwork and its publication in French North-West Africa are deplored and castigated as they deserve. The consequent handicap to any generalized study is felt by everyone attempting an appraisal of the prehistory of this area; but a more scientific approach, to which Professor Vaufrey has himself so outstandingly contributed, is now making itself felt.

The archaeological exposition is laid out in coherent groupings based on age, type of deposit and geographical province. It begins with the Villafranchian site of Ain Hanech in Algeria, possibly of Lower as well as Upper Villafranchian age, with its characteristic fauna and a yield of polyhedral pebble tools (a type with an

extraordinarily long range afterwards), as well as proto-handaxes not yet satisfactorily authenticated as to level. Ain Hanech ranks among the still small world group of archaeological Lower Pleistocene deposits. Correlation with the comparable European deposits is discussed, but we must await Vol. II to learn Professor Vaufrey's views on the tie-up with the earliest East and South African sites.

There follows a searching analysis of the researches by Neuville and Ruhlmann upon the coastal deposits around Casablanca, viewed in the light of subsequent geological and malacological work by Lecointre, Bourcart and others: there emerges a more acceptable version of the course of Pleistocene events in relation to three interglacial transgressions.

Treatment of the later biface and flake cultures, culminating in the Aterian, continues on the same lines of detailed factual information accompanied by a running and often caustic commentary.

A tendency towards rejuvenation, even in the older palaeolithic, is noticeable, and Africa, to a certain extent, is viewed not so much as a retarded but as a retentive continent. On the whole, however, speculation is avoided; and if personal opinions are hoped for upon such topics, for example, as the origin of the older aspects of the Aterian, they may or may not await us in the second volume.

The second main section of this volume is devoted to the Capsian and derivative industries up to the Neolithic of Capsian tradition. This will remain the standard work beyond the day when the question of Capsian origins yields to further research. Radio-carbon tests to date of 6450 B.C. for the Typical Capsian and 5050 for the Upper, have vindicated Professor Vaufrey's long maintained arguments against a Pleistocene antiquity. Without prejudicing the possibility of a still undiscovered Capsian ancestral to the 'Capsien typique,' Professor Vaufrey looks to Western European Perigordian influences travelling to North-West Africa by way of Gibraltar or Sicily; and he rejects provisionally, for distributional reasons based on existing though incomplete knowledge, the probability of an Eastern Mediterranean origin.

G. CATON-THOMPSON

**Egypt: Paintings from Tombs and Temples.** Introduction by Jacques Vandier, Paris and New York (New York Graphic Society and U.N.E.S.C.O.), 1954. Pp. 14, 32 plates. Price £4 10s.

**82** This is an impressive volume in the format of the U.N.E.S.C.O. World Art Series. It consists of a short preface by General Neguib, an introduction by Jacques Vandier, Conservateur-en-chef of the Department of Egyptian Antiquities at the Louvre, three black and white photographs showing temples and tombs, and 32 coloured plates based on photographs specially taken by K. S. Diradour.

Dr. Vandier tells us that 'the intention behind this publication is to remind art lovers that the Egyptians were not only architects, sculptors and craftsmen of genius; they were also painters who were aware from the beginning of the extent to which a drawing or a carving in relief could be improved by colour.' Painting was applied chiefly to the mural decoration of tombs and temples, on a surface which had usually been carved with the pattern first, though many of the plates in this book are from the Theban necropolis where the rock was unsuitable for carving. Four plates are of the Old Kingdom, when at first only the stela was decorated; but, later, figures with offerings were painted on the walls, and, later still, scenes of everyday life. The great flowering of this art was during the XVIII Dynasty so that 15 of the 32 plates are of this period. From them we see many aspects of Egyptian secular life, and can observe the increasing elaboration and luxury, the increasing artificiality of diversions, the increasing degree of civilization approaching the verge of decadence. The XIX Dynasty brings a change of emphasis; secular scenes go out of favour and religious and funerary themes predominate. There are six plates of this Dynasty and seven of the ensuing Ramessid period.

The plates are interesting and well chosen, though their order could have been more systematic. One recognizes the skill of the conventions of Egyptian art which give an added dignity to the human form, and infuse a feeling of elegance into the scenes. Several features of technical interest are to be seen: corrections of line (Plates 12 and 31), the wearing away of the paint revealing clearly the underlying sculpture (Plate 9), and an unfinished mural showing preliminary sketching, finished drawing and carving (Plate 12). It is unfortunate that no indication whatever is given of the size of any of the paintings. There is also a curious editorial lapse at Plate 32, which shows only half the picture described in the caption.

However, it is a pleasant change to have plates of Egyptian paintings reproduced from direct colour photographs instead of from water-colour copies, but unfortunately no details are given of the photographic technique employed, which would help those not well acquainted with the originals to assess the accuracy of the colour rendering. As it is, the marked difference in colour between Plates 25 and 26 which are overlapping photographs of the same painting, does not inspire confidence in the colour rendering of the others. Of course no combination of methods of photography and printing can yet render the originals with absolute fidelity, and these plates are certainly able to give a general impression good enough to satisfy the 'artists, teachers, students and the wide art-loving public' (p. 12), for which this expensive book is intended. FRANK WILLETT

**Monographies Ethnologiques Africaines: Les Bambara.** By Viviane Pâques. 1954. Pp. viii, 123. Price 700 francs. **Les Songhay.** By Jean Rouch. 1954. Pp. vii, 100. Price 600 francs. **Les Coniagui et les Bassari.** By Monique de Lestrange. 1955. Pp. 86. Price 600 francs. All Paris (P.U.F.)

**83** **Ancient American Pottery.** By G. H. S. Bushnell and Adrian Digby. London (Faber & Faber), 1955. Pp. 51, 84 plates, 4 in colour, 2 maps, 1 table. Price £1 15s.

**85** The title is slightly misleading as the book deals mainly with the pottery of the American civilizations at their zenith, and to some extent the pottery influenced by these cultures in the adjacent regions. We lack information, for instance, on the pottery in the region of the Mississippi and on the artistically still higher pottery of some parts of the Amazon region. The limited

These three volumes are the first to appear on French Africa in the *Ethnographic Survey of Africa* series. They closely resemble in layout the very valuable series that has already appeared in English, and differ only slightly in format. Only the third, unfortunately, carries an index, just as in the English series an index was omitted in the earlier volumes. The present works also vary in their bibliographies; all carry extensive and, no doubt, full bibliographies, but the second two are usefully annotated.

It is a further point of importance that these authors have done and continue to do fieldwork among the peoples of whom they write and this, perhaps, partly accounts for the ease of presentation noticeable here and not always attained in the library studies included in the Survey. Again, all three contain brief notes on social change in the modern situation.

Mme. Pâques gives a fuller account of the demography and the economy of the Bambara than is usual, over 40 pages, while social organization (political system, family, marriage, organization of work, inheritance and law) gets only 25 pages. Further, her usage of the terms *chef*, *chef de la terre* and *maitre de la terre*, is a little confusing. However, as she herself points out, the data on this aspect of the subject are poor. The brief statement on religion is admirably carried out, and here the author had the advantage not only of the early work of Tauxier and Monteil, but also the recent studies of de Ganay and, especially, Dieterlen.

In *Les Songhay*, the least satisfactory section is again that on social organization. The structure here called *grande famille* appears to be the *grande famille indivise* of Delafosse and other French writers, or a lineage. Nor is it exceptional in this kind of society, though the author seems astonished at it, that a woman may succeed to the ritual post of 'Owner of the Land.' A number of extended studies of the Songhai by Dr. Rouch have already appeared, and we look forward eagerly to the appearance of his study of their Islamized religion.

*Les Coniagui et les Bassari* is really something in the nature of a brief report on fieldwork to date. The rotation of the houses of a village *pari passu* with the rotation of crops is admirably described, and the marriage system clearly outlined. Further work on this people will be a valuable contribution to the theory of segmentary societies, since they are matrilineal and virilocal.

Finally, fieldwork continues among all these peoples and it will be interesting to follow up these studies. The first two societies are examples of what Nadel called 'symbiotic unions' and the second two are segmentary societies. All pose important theoretical problems.

DAVID TAIT

**The Trade of Lake Victoria.** By V. C. R. Ford. *East African Studies*, No. 3. London (Kegan Paul), 1955. Pp. 66, 7 plates, 8 tables, 11 maps and diagrams, bibl. Price 11s.

**84** This is a geographical study of the region, extremely well done, showing its economic development up to 1952 gauged in terms of traffic carried on the Lake by steamers and dhows.

Apart from statistics the history of achievement is also very interesting and comprehensive, for one gets a picture of the whole area rather than the achievements of each of the three Administrations, Kenya, Uganda and Tanganyika, which border on the Lake.

It is an excellent textbook for teachers of geography in East Africa, or sociologists interested in the changing pattern of the economic life of the inhabitants of this region.

ERNEST B. HADDON

## AMERICA

space at their disposal may, no doubt, have forced the authors to make this regrettable omission.

The two authors have divided the task so that Mr. Digby is responsible for the part dealing with Central America and Dr. Bushnell for that of the pottery of the south-western United States and South America.

The illustrations in the work are excellent both as to printing and as to the selection of the pictures intended to show the characteristic features of the various groups of pottery beyond the information

in the text. The condensed text gives a clear and concise picture of the pottery of the various cultures, of their chronological order and of their relationship to other cultures.

The work of Dr. Bushnell and Mr. Digby is an excellent guide for people wanting to make the acquaintance of ancient American pottery. It is also an instructive book for people wanting to know more of the pottery of the American civilizations at their zenith than they can read on the ordinary labels in museums. A brief bibliography is provided.

S. RYDÉN

**The Functional Body Measurements of School Age Children.**  
By W. Edgar Martin. Chicago (Nat. School Service Inst.),

**86**

1954. Pp. vi, 90. Price \$1

Anthropometry has achieved its greatest development in the United States, where interest in this field has extended over many years and is widespread in the commercial as well as in the academic world. The realization in America of its value as a scientific basis for design is reflected in the survey whose findings are reported in this new publication, for it was sponsored by manufacturers and planned throughout with their collaboration in order to provide new standards for the design of school equipment.

The last large-scale survey in the United States for this purpose was made by Bennett in 1925, and since that time there have been considerable changes in the physique of children. For example, 14-year-old American boys today equal the size of 16-year-old boys of 25 years ago. Few of the more recent surveys have supplied data relating directly to measurements of children in working positions.

The study has been planned and directed by Dr. W. Edgar Martin of the Office of Education with members of the University

of Michigan, who have taken a very comprehensive series of 55 measurements on 3318 Michigan school children aged from 5 to 20 years. All these measurements relate to positions normally taken in school activities and many, such as arm reach in standing, sitting and stooping positions have not previously been made on such a representative sample. They are given in the form of mean, standard deviation and 1-99 per cent. limits of the range for each year of age. Rather surprisingly, thigh length to the bend of the knees, although measured, is recorded only in combination with maximum seat height; seat depth must apparently be calculated from this combined measurement or from total thigh length with an estimated allowance for the distance from the front of the knee to the popliteal fossa.

It must always be borne in mind when using these measurements, that the average American child is significantly taller and heavier at most ages than the average European child. Tables are provided which correlate certain measurements with a range of stature and sitting heights, and may be used where these are known, and differ from those of children in the survey.

No attempt has been made to suggest the anatomical principles which should underlie the use of the measurements, or the degree of tolerance to a working surface set high or low, which might be taken into account when estimating its position. However, the measurement described as maximum seat height has been taken by an ingenious method in the position where full support is given by the ischial tuberosities.

The book is attractively bound and clearly printed, with diagrammatic representations of each measurement, and should be a most valuable source of information to the designer of school equipment.

K. G. PICKETT

## ASIA

**Indonesian Sociological Studies. Selected writings of B. Schrieke.**  
Part I. The Hague (van Hoeve), 1955. Pp. xii, 314. Price  
87 9.50 florins

The late Professor Schrieke, from whose writings these selections are taken, was a professor of law in Batavia until appointed in 1929 as Director of Education in the Netherland East Indies. The papers selected here are four in number, the first of which treats of the 'Shifts in Political and Economic Power in the Indonesian Archipelago in the Sixteenth and Seventeenth century.' This is a detailed examination of historical changes in trade routes, in economics, and in political ascendancy, which took place in Indonesia during the period that began with the rise of Islam there and ended with the stabilization of Dutch power. It studies, for instance, the changes which transformed the Javanese into a purely agrarian people, while the Macassarese on the other hand turned from farming to seafaring, and it provides a very carefully analysed and documented politico-economic survey of Indonesia over that period.

The second paper studies the causes and effects of the spread of communism on the west coast of Sumatra, where it is interesting to note a number of factors which have influenced social change in parts of India in closely similar directions. Thus the introduction of a money economy has led to a growing individualism, to the break-up of matriarchal family ties and to the weakening of the authority of local chiefs and headmen. Wealth has tended to change hands and to pass from the more stable and responsible members of society to the irresponsible and uncontrollable. The development of internal communications and the centralization of political power have reinforced this tendency. This has afforded communist agitation a convenient opening, while the administration, distrustful as it was of all innovation, failed to make the use it could have made of the anti-communist views of religious reformers. All ranks of society had grievances—the traditional leaders of village communities because the respect and esteem which used to be shown for them was dwindling; the newly rich and newly educated because although feeling superior in knowledge and substance to those leaders they still had to regard them as their betters; the labouring man because he felt it an injustice that he did not share the growth in power which accompanied wealth.

The third section deals with the native rulers of the Netherlands Indies and explains the change in their position as a result of Dutch

administration, which virtually turned into unsalaried officials the natural leaders of the people, whether they were village headmen of the Toradja type, whose authority depended on acceptance and recognition by their fellow villagers, or were hereditary princelets of the pattern found in the coastal areas.

The fourth paper consists in 'Some Remarks on Borrowing in the Development of Culture,' in which the author demonstrates the effects of changes in trade, and in goods traded, in bringing about social changes. In this, as indeed in all four papers, the author justifies the position which he adopts in his introduction that the Indonesian archipelago, though taking an important place in the theories of the so-called 'historical,' i.e. 'diffusionist,' schools of ethnology, both German and English, is in fact the area above all others in which the fallacies of these schools can be demonstrated.

There are three maps, but a little more information on them would be welcome; Kuantan, for instance, referred to several times, is not to be found on any of them. The format of the volume is attractive, and though printed in a foreign press has the minimum of misprints in English spelling. The translation, the work of four different hands, reads excellently and only a single eccentricity in choice of words reveals the fact that it was not written in English. The editors are to be congratulated on a very scholarly production.

J. H. HUTTON

**Without the Chrysanthemum and the Sword. By Jean Stoetzel.**  
London (Heinemann) and Paris (U.N.E.S.C.O.), 1955. Pp.  
88 334. Price 16s.

The author, a French sociologist who knows no Japanese, was sent to Japan by U.N.E.S.C.O. 'to study the attitudes of contemporary youth,' in particular to the recent reforms, the object of which is 'to reduce the excessive pressure of authority on the individual.' She was accompanied by a Dutch expert on Japanese culture, and received much help from Japanese investigators. The enquiry was conducted mostly by questions and questionnaires put to university students and others.

In her investigations she had to allow for the fact that 'Japanese culture has no real place for the concept of individualism. Many of the antitheses familiar to the West—freedom or slavery, good or evil, happiness or misery, democracy or totalitarianism—are meaningless when applied to Japan' (p. 63).

Some conclusions are—that Japanese youth takes more interest in international problems than in those of Japan (p. 111); that while there are many genuine pacifists, 'many undoubtedly sincere pacifist opinions are seen on analysis to be superficial and sentimental, easy to upset' (p. 133). This is particularly true of the women, who are more traditional and less social (p. 175). 'Attachment to the Imperial House remains strong. This attachment is, however, much more political and philosophical than personal' (p. 159). In theory, most are in favour of sex equality, 'but as the question becomes more practical and its implications more

personal, it gives rise to astonishment, indecision and finally withdrawal' (p. 177). The official Shinto cult has been suppressed. It is uncertain to what extent it is still believed in, but 'it appears that what we call religious needs, while not absolutely unknown to the Japanese, are an exceptional element in their psychology' (pp. 191-2).

We cannot be sure to what extent the 80 tables and 73 pages of answers to questions represent the genuine views of the Japanese, but the author is to be congratulated on having dealt with a difficult assignment in a very competent manner.

RAGLAN

## CORRESPONDENCE

**Indians in Africa.** Cf. MAN, 1956, 24

**89** SIR.—May I make a few additions to Dr. Homburger's very interesting article, 'Indians in Africa'?

Though the 'land of Punt,' to which Queen Hatshepsut sent her expedition, was undoubtedly a port on the African coast, this single example cannot be regarded as conclusive as to the exact position of Punt. There had been many earlier expeditions to Punt, and there are references to the 'lands' of Punt, showing that there were other places to which the name was applied. The products of Punt were very varied and include many which are not African. The perfumes of Punt were proverbial in Egypt, and it is in India and Ceylon that the necessary ingredients for perfume—spices and heavily scented flowers—were found in abundance. (The modern idea that perfumes originated in Arabia is probably due to the fact that Arab traders introduced them into Europe.) Scented wood was also a product of 'Punt,' and sandalwood and camphor wood come from the East Indies. The hunting leopard (cheetah) is Indian, not African, yet Hatshepsut acquired a pair from her land of Punt.

The facts suggest that Punt (the *t* is merely the feminine ending and is therefore not part of the root) is a generic term for a trading port, where traders met and exchanged goods. I venture to suggest (and here I feel that I, not being a linguist, exhibit a temerity almost amounting to daredevil recklessness) that Pun is perhaps from the same root as, or even the origin of, Punic and Phoenician, and has the meaning of 'littoral,' lands to which seaborne trade could be carried.

There are two other points which show a connexion between India and Egypt in ancient times. The first is a rapid sketch of a jungle cock drawn in ink on a potsherd, dating to the late eighteenth dynasty. All our modern cocks and hens are derived from the wild jungle fowl of India, and this sketch is proof that these birds were already known in Egypt in the second millennium B.C.

The second point is to my mind important as showing a unity of ideas. In ancient Egypt and in ancient India, and as far as I know in no other country, both the cobra and the lotus were endowed with peculiar sanctity.

London, W.C.1

MARGARET A. MURRAY

**The May Tabu on Marriage.** Cf. MAN, 1956, 37

**90** SIR.—Professor Davis says that 'The origin of the cult of Vesta is of great interest—it lay in the necessity in the primitive community of keeping one fire always alight, so that its members could at any time supply themselves with it as it was hard to obtain it at a moment's notice. The task of keeping the fire alight was that of the chief's daughters.'

This theory is not new, but there are many reasons for regarding it as highly improbable. Among them are:

- (1) Savages can, and often do, make fire quite easily.
- (2) Chiefs' daughters never are, and are never likely to have been, employed as menial servants to the community.
- (3) Sacred fires, lamps, etc., are sacred, and are never allowed to be used for secular purposes. Fire is only taken from them ritually, as at the annual rekindling.
- (4) Professor Davis's idea that the sacred hearth was an insurance for careless housewives against being late with their husbands'

breakfast is incompatible with the rest of his thesis, according to which it formed part of an elaborate ritual of purification.

(5) There is no good ground for believing that any rite had a utilitarian origin.

Usk, Monmouthshire

RAGLAN

**Iron Gongs.** Cf. MAN, 1955, 30, 196; 1956, 20

**91** SIR.—James Walton refers in his recent letter to a double gong formerly owned by 'Chief Mulungwe of the We' and now kept in the Rhodes-Livingstone Museum. This is incorrect and should read 'Chief Mulungwe of the Lala.' The mistake was due to a labelling error in one of our showcases which was only discovered and rectified after Mr. Walton's visit. Both the Curator and I deeply regret thus misinforming Mr. Walton and trust that he will accept our apologies.

Rhodes-Livingstone Museum,  
Livingstone, Northern Rhodesia

B. REYNOLDS  
Ethnographer

**For a New Definition of Marriage.** Cf. MAN, 1955, 198, 199;

**92** 1956, 46

SIR.—The new definition proposed by H.R.H. Prince Peter is not a real definition and cannot help the field-worker. He defines marriage as: 'The socially recognized assumption by man and woman of the kinship status of husband and wife.'

The Oxford English Dictionary tells me that a husband is a 'man joined to a woman by marriage,' and that a wife is a 'married woman esp. in relation to her husband.' In Webster I read that a husband is 'a man who has a wife;—the correlative of wife,' and that a wife is 'a woman united to a man in lawful wedlock; a married woman; spouse;—correl. of husband.'

Thus Prince Peter's definition amounts to this: marriage is 'the socially recognized assumption by man and woman of the married status.' The poor fieldworker studying a tribe that does not know the English word 'marriage,' and finding there two forms of socially recognized mating, cannot settle the point whether the partners concerned are each other's 'husband' and 'wife,' since they do not know these words either. The partners refer to each other as 'a' and 'b,' or 'x' and 'y,' and the fieldworker does not know how to translate these terms into English. Are the women 'b' and 'y' wives or perhaps mistresses or concubines?

H. TH. FISCHER  
Volkenkundig Instituut van de Rijksuniversiteit, Utrecht

**'Utrilateral' and 'Utralocal.'** Cf. MAN, 1956, 30

**93** SIR.—You will, I think, not frown on my coming to the defence of the two words 'utrilateral' and 'utrolocal' against which Dr. Needham protests in his generous review of my report on Iban agriculture. For, as I gratefully recollect, you were one of their begetters.

Dr. Needham has two complaints. First (having said them over quickly) that the words are 'horrid'; and, second, that there are already terms (in ambilateral and ambilocal) that make them superfluous. Let me deal with his second point first, for it is the one, certainly, that matters most.

The neologisms to which Dr. Needham objects have been employed because in my judgment Iban family structure possesses

certain features which do require new and specific terms to describe them. I had better begin by explaining briefly what these features are.

Every individual in Iban society is born into one particular family (or *bilek*), and this is just as likely to be the family of the child's father as it is to be the family of the child's mother, for virilocal and uxorilocal residence occur with very nearly equal incidence. Thus, the analysis of an extensive series of Iban marriages, for all of which reliable information was available, shows that virilocal residence occurs in 49 per cent. of cases, as against uxorilocal residence in 51 per cent. of cases.

The family of which a child is a member by right of birth, and in which he (or she) grows up, we may term his natal family. It is to this family—and this family alone—that he belongs, and he remains a member of it always, unless he be adopted or, upon reaching adulthood, he marries out into some other family. Further, membership of his (or her) natal family confers upon a child inheritance rights over its properties and lands, and these rights are retained as long as the individual concerned remains a resident member.

The family in which an individual grows up, then, and of which he is a member by right of birth, may be either the family of his mother or the family of his father; but one or the other it must be. He cannot belong to both of these families at the same time. This means that among the Iban, filiation is of a special kind. For to reiterate, it may be either to an individual's mother's family, or to an individual's father's family, but it cannot be to both at the same time. Moreover, in practice both types of filiation occur to an approximately equal extent. We are here confronted with a fundamental principle of the Iban family system.

When I uncovered this principle, I at once looked about for a suitable term to apply to it and the term which I first tried out was ambilateral. But I discovered on trial that ambilateral just would not do. Let me explain why.

Ambilateral, it will be remembered, was brought into use by Raymond Firth in 1929, to describe the *hapu* of the New Zealand Maori. Of the Maori *hapu*, Firth wrote: 'Descent through one parent only was necessary to establish membership in it, but an interesting feature of the system is that both mother and father were counted in tracing descent. Were they of different *hapu*, the child belonged to both . . .' (Raymond Firth, *Primitive Economics of the New Zealand Maori*, London, 1929, p. 98; my italics). Furthermore, Firth made it clear that it was possible for an individual to hold rights (*i.e.* over land, etc.) in two *hapu* at the same time (*ibid.*, p. 100). Raymond Firth, then, called the Maori *hapu* 'an ambilateral group, since both parents are eligible for the purpose of kinship affiliation' (*ibid.*, p. 98).

This usage of Firth's is perfectly clear and straightforward, and it conforms to the proper and accepted meaning of ambilateral, *i.e.* 'pertaining to or affecting both sides; bilateral' (Webster's *New International Dictionary*, London, 1934).

Let us now return to the Iban. The Iban system of filiation is specifically *not* ambilateral, and to refer to it as ambilateral would be to misconstrue completely the principle on which it is based. Whereas, under an ambilateral system, filiation may be to the families, etc., of 'both mother and father,' under the Iban system this is precisely what cannot and never does happen. Among the Iban—let me state again—filiation may be *either* to the maternal family *or* to the paternal family, but *never to both*. I hope that the considerations which led me to abandon ambilateral as a term which could be correctly applied to the Iban system will now be apparent.

But this still left me with the problem of finding a word that might be used to describe the Iban system. After not a little thought, I was driven to the conclusion that the only solution would be to coin a new word. (In doing so, let me assure Dr. Needham, I acted not out of obsessive-neolalia, but in the conviction that I was dealing with a hitherto unrecorded regularity of human behaviour that deserved a clear and distinct name.)

It was at this juncture, Sir, that I wrote to you, and our correspondence on the problem of devising a fitting term began. Eventually we decided that the best course would be to base our neologism on the Latin *uter* (C. T. Lewis and C. Short, *A Latin Dictionary*, Oxford, 1907: 'Uter—either of two, one or the other, one of two').

for it would then denote unequivocally the principle which underlies the Iban system of filiation.

By the term *utrolateral*, then, we mean to denote a system of filiation in which an individual can possess membership of either his father's or his mother's birth group (*e.g.* the *bilek* family among the Iban), but not of both at the same time. Utrolateral is thus fundamentally different from ambilateral in its connotation, for as has been shown, ambilateral refers to a system in which an individual can possess membership of, and hold rights within, both his father's and his mother's birth groups, etc., at one and the same time.

(The forms 'utrolateral' and 'utrolocal,' it should perhaps be remarked, were chosen in preference to 'utrilateral' and 'utrilocal,' so that there might be no possible confusion with uterine, utricular, etc. This may not have been, on etymological principles, an entirely sound decision, but it was one which excited no particular qualms in the Latinists to whom it was submitted for comment.)

All neologisms are, I agree, to some extent 'horrid.'

In words, as fashions, the same rules hold,  
Alike fantastic, if too new or old.

But sometimes one must be the first to use a word. I hope, Sir, that I have shown that the Iban system of filiation does deserve a term of its own, and that I may be allowed, perhaps, to keep my word.

*Australian National University, Canberra*

J. D. FREEMAN

#### A Cameo Found on a Beach at Conakry. With a text figure

Sir.—The collections of the Institut Français d'Afrique Noire at Dakar have been given a pebble, perhaps a soapstone, 3 inches high and 2 inches wide, sculptured in cameo with a representation of an ancient Greek warrior. It was found, according to the donor, on a beach of the town of Conakry,



FIG. 1. A CAMEO FROM CONAKRY (ACTUAL SIZE)

the capital of French Guinea. The discovery raises, evidently, the problem of whether the sculpture is genuine, ancient or only a modern reproduction? Even in the first case, a recent introduction could probably not be ruled out, in the present state of our knowledge. An important point would be to discover whether such objects were still being made in certain countries (for example, Italy) for sale as curios. Any information would be very gratefully received.

I.F.A.N., Dakar

RAYMOND MAUNY





A LIFE-SIZE TERRA-COTTA HEAD FROM NOK

*Height: 14 inches. Photograph: B. E. B. Fagg*

# A LIFE-SIZE TERRA-COTTA HEAD FROM NOK\*

by

BERNARD FAGG

*Jos Museum, Nigeria*

**95** The terra-cotta head illustrated in Plate G was discovered in the summer of 1954 in the course of tin-mining at Nok in southern Zaria Province, Northern Nigeria, by a workman employed part-time by the Jos Museum. This head, which was found face downwards in a narrow channel on the bedrock and overlain at this place by about 12 feet of gravels, clay and sands, is exactly typical of the epineolithic Nok Culture,<sup>1</sup> whose area is now known to stretch approximately 300 miles across the Benue-Niger valley from Katsina Ala in Benue Province to Kagara in Niger Province. The extent of this culture appears likely to be very much greater, for so far the majority of specimens have been found during mining in alluvial tin-bearing deposits of a depth varying from a few feet to the 42 feet of the stratified main deposit at Nok, where most of the specimens have been discovered.

Associated finds at this and many other sites at Nok and elsewhere include polished stone axes, an iron axe of identical form (corroboration for at least three hafted stone or primitive iron axes depicted on figurines), *tuyères*, slag, furnace waste, grooved grinding stones for both axes and beads, perforated quartz beads, lip plugs and other ornaments, fine-quality domestic pottery including potstands, at least the probability of tin beads, many of which have been found though not yet during controlled excavations, and a single stone bead mould for casting tin beads. In addition there is much internal evidence of the material culture in the terra-cotta figurines themselves, many of which show a wealth of carefully executed details of dress and accoutrements comparable to some of the work of Ife and Benin.

The age of this culture is still tentatively placed at the end of the first millennium B.C., on the basis of geomorphological evidence, and this is not improbable on cultural grounds. Excellent carbon-14 samples (which are certainly contemporary with the figurines) have been submitted for analysis four and a half years ago, but the inevitable difficulties which have delayed so many important determinations have not yet been overcome. There is also a possibility that a single pale green glass marble from one of the mining excavations at Nok may assist in dating this culture. This is the fourth such specimen known to have been found during the last 50 years in the Plateau minesfield.

The present head, which is of natural size or slightly larger, is evidently part of a large figure, and is a very creditable achievement considering that it was probably

pile-fired. The white flecks are fragments of quartz and felspar grog from crushed granite, a technique still practised by the Jaba tribe at Nok. The fabric is about three-eighths to half-an-inch thick, and great pains were taken to see that this thickness was maintained throughout, e.g. at the neck, where the necklace of beads or plaited fibre has been made hollow. The interior shows finger marks all over, whereas the exterior has been carefully tooled to bring the fine clay to the surface. The everted lips, especially the lower one, and the tip of the nose have been damaged by abrasion.

The five 'buns' of hair resemble the present-day hair style of the Kachichiri and Numana, tribes living about 30 miles east of Nok. Four of these buns have holes which may have been used for the insertion of feathers—an inference from contemporary practice. Three strings of beads or plaited fibre across the forehead end at the temples, where something has been broken away at each side. The splayed nostrils, flat bridge of the nose, everted lips and triangular perforated eyes with sweeping semicircular eyebrows are typical of the Nok style. The keeled ornaments on the buns of hair are probably plaited hair, though no modern parallels are known to me.

The rest of the figure is missing and there is only a remote chance of finding further fragments. If it was a standing figure—and legs and feet have been discovered which could support such a statue—it could hardly have been less than four feet high.

This head suggests that the makers of the Nok figurines were physically as well as culturally closely parallel to the many pagan tribes living in Northern Nigeria today. There may in fact have been an extensive cultural unity, owing its existence perhaps to the introduction of metallurgy, from which many of the very diversified present-day pagan cultures of the Niger-Benue region have derived.<sup>2</sup>

## Note

<sup>1</sup> For some account of the Nok Culture, see my 'Archaeological Notes from Northern Nigeria,' *MAN*, 1946, 48, and articles published in the *Illustrated London News* for 26 April, 1947, and 20 November, 1948; also 'A Preliminary Note on a New Series of Pottery Figures from Northern Nigeria,' *Africa*, Vol. XV, No. 1 (1945), pp. 21-22, and 'An Outline of the Stone Age of the Plateau Minesfield,' *Proc. III Internat. Afr. Conference, Ibadan, December, 1949*, Lagos (in the press).

<sup>2</sup> Professor J. H. Greenberg, who was conducting linguistic studies in the Jos Plateau and surrounding regions during 1954 and 1955, told me in conversation that there probably existed a 'proto-language' in this area about 2,000 years ago (very approximately), of which traces can be found in the basic vocabularies of many of the present-day tribes.

## RITUALIZED PERSONAL RELATIONS

BLOOD BROTHERHOOD, BEST FRIENDS, COMPADRE, ETC.: SOME COMPARATIVE HYPOTHESES AND SUGGESTIONS

by

DR. S. N. EISENSTADT

*The Eliezer Kaplan School of Economics and Social Science, The Hebrew University; and The Center for Advanced Study in the Behavioral Sciences, Stanford, California.*

**96** The purpose of this paper is to present a set of tentative hypotheses on the social conditions in which a certain type of institutional behaviour, which at this point may be called 'ritualized personal relations,' exists and functions. Under this term I shall include a variety of phenomena such as blood brotherhood, blood friendship and 'best' friends, compadre relations and the godparent relation in several peasant societies, as well as relations of contractual servanthip existing between members of different castes in an Indian village,<sup>1</sup> or the Kan-shing relationship reported by Fried in China,<sup>2</sup> which perhaps do not come at first sight under this general category. My general contention will be that all these relations have some basic characteristics in common, although they vary in the intensity of these characteristics, and that these characteristics are related to some similar or parallel social conditions. With the exception of B. Paul's unpublished thesis,<sup>3</sup> most of the work on these subjects has been confined to one or two types. Thus, blood brotherhood and best friendship have been lately summarized by Tegnæus and Gibbs,<sup>4</sup> while Mintz and Foster have summarized and analysed the material on compadre.<sup>5</sup> The various ethnographical papers which report these data are numerous, but usually contain very little comparative analysis.<sup>6</sup>

While many of these analyses show much insight, it is nevertheless my feeling that most of them do not analyse in a systematic enough way the conditions in which these types of relations exist. Nor do they recognize sufficiently—if at all—some of the *basic* characteristics and conditions which are common to most of these phenomena.

In this paper, no new data will be brought forth. Its only purpose will be to present, on the basis of existing data, some systematic hypotheses concerning the conditions in which this type of behaviour exists and the functions which it performs in its social setting. In this way perhaps some lacunæ in the data as well as some further possibilities and directions of research may be indicated.

### *Some Basic Social Characteristics of 'Ritualized Personal Relations'*

I may perhaps begin by pointing out the common basic analytical characteristics of all these relations. These seem to be four—they are particularistic, personal, voluntary and fully institutionalized (usually in ritual terms). By particularistic I mean that the incumbents of the relationship act towards one another in terms of their respective personal properties and not in terms of general universal categories.<sup>7</sup> In this they are very close to kinship relations

and groups (which are also predominantly particularistic) but unlike the latter in that they are incurred in a voluntary way. Thus, those who participate in these 'brotherhood,' etc., relations in this case do not do so by virtue of their 'hereditary' positions but are chosen by each other.<sup>8</sup> Unlike many other voluntary, contractual relations, however, these relations, because of their particularistic connotations, are not 'anonymous,' i.e. directed towards universalistic categories of people, but are very personal and intimate. On the other hand, unlike many personal relations both in those societies and in our own, e.g. various types of personal friendship, of clique membership, etc., these relations are usually fully institutionalized, sanctioned by some of the most important and severe, usually ritual, sanctions of the society. This is true both of the pure types of 'ritual kinship' and of the 'looser' types of relations like the Kan-shing, etc., although in the latter cases, the extent and duration of mutual obligations is shorter and the severity of sanctions is weaker.

In order to understand more fully the nature of these relationships, we should also inquire into the contents and nature of the obligations that they incur. The fact that these relations are both voluntary (hence to some extent contractual) and also particularistic and personal indicates that we have here some interrelation between solidary and instrumental obligations. In all such relations there exists a set of mutual obligations in the instrumental and economic fields. Some such obligations seem to recur in most of these cases—e.g. mutual help in cases of economic hardship, illness or other calamities, some sort of mutual insurance in common economic enterprises, participation in funeral expenses, participation in some costs of educating children, etc.<sup>9</sup> In some cases the extent of mutual obligations is much broader, including general hospitality, sometimes up to sharing of wives,<sup>10</sup> help in litigation and against the demands of powerful men,<sup>11</sup> while in others it may be narrower. But some instrumental obligations do always form a basic component of this type of relationship. Therefore, Gibbs's contention that this type of relationship is purely 'affective' and non-instrumental is not correct.<sup>12</sup> There is, however, an important element of truth in his contention and this is related to the fact that all these mutual (or, as sometimes in cases of godparent–godchild relations, unilateral) instrumental obligations are set within a framework of diffuse solidarity. These obligations are not defined as stemming from some specific, limited contractual, market-type commitments and relations, nor are they set in terms of universalistic categories of people.

They are set in terms of diffuse, solidary relations, symbolized usually in some ritual act, and it is in terms of these relations that the instrumental obligations are defined. At the same time, however, the instrumental obligations are not of secondary importance or only accidental derivations of the solidary relationship. Sometimes (probably very often, but the data are not adequate on this) the main, and even explicit, reason for contracting this relationship seems to be these potential instrumental benefits. We have here then a peculiar and distinct type of combination of instrumental and solidary relationship, in which the solidarity provides the basic framework, yet within this framework various instrumental considerations, albeit very diffusely defined, are of paramount importance.

It is this combination of solidary and instrumental relations that may provide us with a starting point for the discussion of the conditions in which these types of relations develop and exist.

#### *The Social Conditions in which Ritualized Personal Relations Exist*

I should like to propose the hypothesis that such conditions arise mostly in some types of predominantly particularistic societies and are related to some tensions and strains inherent in these societies. By predominantly particularistic societies I mean societies in which (a) the incumbents of the most important roles act towards other persons according to the familial, kinship, lineage, ethnic and other properties of those individuals in relation to their own, and (b) membership in the total society is defined in terms of belonging to some particularistic sub-group (lineage, caste, etc.), and the most important institutional roles in the political, economic, ritual, etc., spheres are allocated to such groups or their representatives.<sup>13</sup> It seems to me that in such societies there exist two main areas of strains and consequent problems of integration. The first are strains which exist within the basic constituent groups of such societies (lineages, clans, castes, some territorial groups, etc.) and which are inherent in the structure of these groups. Second are the various strains and tensions between the main groups and categories of people of which such societies are composed, and the consequent problems of their integration.

#### *The Internal Tensions within the Main Sub-groups of Particularistic Societies*

Most of these tensions can be related to the fact that in such groups, by the very nature of their organization, there usually is but little specification of the allocation of the various major obligations within the group or category. These are left—at least on many occasions—to the internal arrangements of the particular group. Sometimes these relations may be structured and clearly defined—quite often in ways which may increase the tensions (as in the rigid prescription of seniority rights)—but quite often they may be left to some internal unspecified arrangements of the group involved. These may enable the exercise of many pressures, of bargaining and of illegitimate power, etc. Moreover, it is not always specified, and in many cases

cannot be specified by the very nature of the case, which of the members of a group will start a chain of activities or become involved in some problems or conflicts, which may then affect the whole group. Thus it can never be known who will start a particular feud which may involve the group or some of its members, or who will engage in various commercial or economic activities with other groups, etc., or who will show more initiative or ambition in any field of organization, etc. While it may perhaps be easy to identify such persons in terms of individual idiosyncrasies and characteristics, these do not necessarily coincide with any definite structural positions within the group or within a broad category of people. Thus the very nature of such particularistic groups, with their emphasis on diffuse solidarity (sometimes also on seniority, etc.), lends itself to easy manipulation by certain people who may easily go beyond what seem to other persons to be their legitimate rights. This is especially so in those areas of life and in those enterprises which involve not fully structured intergroup relations, which are not entirely contained within the group and regulated by it, and which yet may easily involve the group itself and many of its members by the very existence of clear definition of situation.

An additional factor of strain in such societies is the possibility of conflicting claims and pressures from different groups or categories of people or any persons, and the limiting of the area of the individual's choice or private life through such pressures. This seems to be especially important in problems of inheritance, the amount and type of property that one can bequeath according to his own wish, etc.<sup>14</sup>

#### *Problems of Intergroup Relations and Tensions*

Generally speaking, there may exist two main areas or types of intergroup tensions in these societies. One type of tension may arise from various strains engendered through close relations and obligations which exist between these groups, relations and obligations which are necessary to their existence and which may yet cause difficulties because of the very strong solidarity of each of these groups. The second type of tension may arise in those particularistic societies in which the relations between such groups are very ephemeral and which need therefore some mechanisms through which regulated relations between their members could be maintained. In all such societies there exists some degree of interdependence and interrelationship between the different sub-groups, in different institutional spheres, ritual, economic, political, etc.<sup>15</sup> The exact spheres in which such interdependence is greatest and the extent to which it is organized differ, of course, from one society to another. But in all cases such interrelationships include certain patterns of mutual obligations, entailing many types of duties in the instrumental field. These may be connected with economic exchange necessitated by marriage and bridewealth arrangements, or with various complementary economic functions between clans, etc. Or there may be obligations to provide manpower for public works or for military and

political purposes, etc. In some cases they may entail such relations between potentially hostile groups which nevertheless have to cooperate in some areas of life and common interest.

It seems to be characteristic of most of these relations and obligations, especially in the instrumental field or in political relations (as, for instance, among the Azande), that they are not entirely 'contained' and regulated by the particularistic criteria and relations of each of these groups and by what may be called their routine interrelations. Many of these mutual obligations are not clearly and concretely defined simply because they are not stated in general, universalistic and specific ways but rather in more diffuse and particularistic terms, and are limited within the framework of diffuse solidary relations. Thus the exact demands that a noble can make of a commoner among the Azande, or a member of a superior caste among the Tanala towards a member of a lower one, are not usually clearly defined and may give rise to a lot of 'private' interpretations and extortions. The same seems to be true of the demands of members of senior age groups towards younger ones (see Driberg, *op. cit.* in note 6), or of the demands between certain family groups in the spheres of intermarriage, etc. This is especially so in cases of eruption, or continuous existence, of hostile relations between such groups or their members. In such cases the exact definition of mutual obligations, compensation, etc., may be a very vexing and uncertain problem.<sup>16</sup>

In other words, it seems that the very kind of social organization existing in such societies creates some types of undefined situations or types of situations of potential conflict malintegration. These situations may be of either of the two main types mentioned earlier. They may arise in those cases wherein the internal solidarity of each of such sub-groups may come into conflict with various exigencies of cooperation and interrelationships between such groups. Or the extent of interrelationship between such different sub-groups may be so loose and small that it is difficult to maintain any regulated relations between them or their members.

Many of these strains and tensions have been analysed in anthropological literature. Various social mechanisms which deal, as it were, with these problems have been pointed out. Among these the most important are the various mechanisms of kinship-extension, of kinship as opposed to lineage obligations, of various types of so-called associations, of rules of hospitality towards strangers, of joking relationships, and generally of various ties cutting across different groups and categories of people.<sup>17</sup>

It is our basic hypothesis that the various forms of ritualized personal relationships constitute also a mechanism of social control which tends to mitigate some of the tensions and strains of predominantly particularistic societies analysed above.

#### *Situations of Strain and Ritualized Personal Relations*

The evidence available at this stage of research is as yet inadequate for a full analysis of the exact relations between the various types of mechanism mentioned above and the

various types of ritualized personal relations. It is thus as yet difficult always to say exactly when the different mechanisms are fully adequate, when they generate new types of tensions and strains and to what extent one mechanism helps in the alleviation of tension created by another. This has to be left for further research. But it is perhaps possible to put forward, in a very tentative way, some propositions as to the nature of the more specific strains with which ritualized personal relations seem to deal. They seem to be most closely related to those situations in which the internal strains engendered within the different sub-groups of these societies are connected, in some way, with some of the intergroup strains, the last reinforcing the first. Thus, among the Didinga, the internal solidarity of an age group may become strained because of demands made by a representative of a senior age group on some member of the junior age group and the latter's demand of help from his age mates. Among the Azande, the several individual members of a family may become involved by one of their members in disputes with the nobles, etc. In Dahomey and among other West African groups, sharp conflicts over the testamentary disposal of the property of an individual may arise between his family and other kinship, etc., groups, and pressures may be exerted on him by the group which can claim his greatest formal allegiance. Illustrations of this sort can easily be multiplied. What seems common to most of them is that some incompatibility develops between an individual's instrumental obligations, his solidary obligations, and his predispositions in these spheres. His solidary relations to some groups or categories of people may become strained because of conflicting or illegitimate claims in the instrumental field made by members of such solidary groups, and/or he may be incapable of fulfilling his instrumental obligations and aspirations because of such solidary claims or because of lack of what to him seems adequate support from his solidary groups. It may therefore be a plausible suggestion that this type of strain is most likely to occur in those situations in which both problems of intra-group solidarity and of intergroup relations arise. In other words, in such situations there arise problems both of societal integration and of individual tension and adjustment.

It is because of these various characteristics of situations of strain that the different types of ritualized personal relations are so closely related to them. The basic characteristics of ritualized personal relations, analysed in the first part of this paper, enable these relations to mitigate precisely these types of conflicts. In order to avoid imputation of any teleological implications in this argument it should be stated at the outset that we do not assume that, whenever such tensions exist, these (or any other) mechanisms of social control always develop, nor that these are necessarily the only types of such mechanisms which could perform this function (for a fuller discussion of these methodological problems see my *From Generation to Generation*, *op. cit.*, chapters I and VI). But we shall come back to these problems later when discussing possibilities of further research.

A closer analysis of the different types of obligations

incurred through relationships of ritual kinship, as well as of its voluntary and personal nature, will illustrate the way in which these relations tend to mitigate the above-analysed tensions, from the point of view of both the individual and the social structure.

As has already been indicated, most of these obligations contain some element of insurance against unexpected risks and calamities. These may be cases of illness, of death, of unexpected sudden economic demands, hardship, etc. In still other cases we find some sort of assurance of safe ways in strange and hostile parts of the country.

But in all these relations there exists an additional basic element—namely, that the performance of these obligations is assured through a special personal bond which transcends the usual existing groupings and categories of people and cuts across them. This bond is usually seen as no less binding—sometimes even more—than that with the categories and groups. In some cases, this bond is more or less expressly oriented to assuring the individual's will as against possible pressures from different groups. This is most clearly seen in Dahomey where the best friend is the executor of an individual's will.<sup>18</sup>

In general it is true, as Gibbs has rightly stressed, that these ritualized personal relations provide the individual with very strong bonds of personal-emotional security—a bond which is of special importance in the various situations of strain and tensions analysed above. From the point of view of social integration, these relations may help in mitigating the potential conflicts or tensions between different sub-groups, family groups, lineages, status groups, castes, etc., of these societies. They furnish an additional tie which cuts across existing corporate groups, and provides the individual with some security and defence from the pressures of his group while at the same time also enhancing the individual's interest in the maintenance of smooth relations between different groups. In those societies in which the main problem is that of alleviating tensions between too closely related sub-groups, such ritualized relations may help in mitigating these tensions by providing various cross-cutting ties. In the more loosely integrated societies they may provide for some regulation and stabilization of intergroup relations.

The available data seem to support our suggestion and hypotheses about the type of social conditions and strains to which various forms of ritualized personal relations are connected and which they help to mitigate. Truly enough, at this stage it is difficult to relate these relations to other types of mechanisms, such as joking relations. But at least we have been able to indicate in a general way the place of these relations within the framework of such mechanisms.

Additional support for our hypothesis can be found in an examination—necessarily rather cursory—of the variability and distribution of these relationships. While the data on this point are not always clear and systematic enough, and while only in very few reports do we find a full statistical analysis of the distribution of these types of relationships between the members of different groups in the society, yet some general indications can be adduced from some of the existing data. One thing that strikes us

immediately is that in different societies these types of relationships are not always established between the members of the same groups. Thus, among the Didinga, as well as among the Plains Tree, they seem to be especially important between the members of different age groups or military-warrior groups. Among the Azande they are limited to the commoners, while among the Tanala they seem sometimes to cross 'status lines.'

Mintz and Wolf have shown that the distribution of the compadre relationship differs greatly in several Latin American communities. They have summarized the data in the following way:<sup>19</sup>

The mechanism may be contrasted, then, in several distinct contexts. In the first context are Tusik, Barrio Poyal and Pascua. These communities are alike in their 'homogeneity,' and the horizontal structuring of the compadre system; yet they are markedly different in other respects. Tusik is tribal and essentially isolated from the world market, while Barrio Poyal and Pascua are incorporated into capitalistic world economics, and are fully formed working class strata. In the second context is San Jose, with its varied land ownership pattern, its mixed (cash and subsistence) crop production and its several classes. Through the vertical phrasing of its compadre system, San Jose demonstrates a relatively stable reciprocity, economic and social, between the landed, large and small, and the sharecroppers and laborers.

In the third context is Moche. Land is held predominantly in small plots; the crops, as in San Jose, are both cash and subsistence, and while Gillin doubts the existence of classes, certainly the compadre system is described as a vertical structuring one. Here, too, the elaboration of face-to-face ceremonialism may help to slow the accelerated trend toward land concentration, a cash economy, and incorporation into the world market.

A closer analysis of these differences will show that they can be explained in terms of our hypothesis. In all these cases the different types of 'ritual kinship' and compadrazgo are usually established between the members of those groups between whom there exists a relatively wide scope of relatively unstructured interaction, or, in other words, they are established in those areas of interaction which may give rise to certain undefined situations and consequent tensions. Not all the interactions and interrelationships between different particularistic groups in these societies are of equal importance from this point of view. Sometimes the relations may be purely on a symbolic plane with relatively little interaction in the instrumental field. In other cases the hierarchical relations between different status groups may be so distant, either in symbolic terms or in actual common interests, as to involve but little common, equal meeting points in instrumental relations. This seems, for instance, to be the case in the commoners-nobles relation among Azande. In all such areas there do not seem to develop any types of ritualized personal relations. Mintz's and Wolf's analysis of the development of different types of such relationships in the Middle Ages seems also to substantiate this point.<sup>20</sup>

Moreover, the exact contents of the obligations within these types of relationships seem also (in so far as clearly reported in the materials available) to vary according to the nature of the specific sphere of potentially unregulated and undefined situations and interrelations. Thus, among the Didinga, this seems to apply mostly in the field of military

duties and behaviour; among the Azande in defensive relations of commoners against the nobles; among some Africans in semi-commercial relations between travellers of different clans; in Dahomey in economic enterprises, etc.; among the various Latin American groups in the fields of economic relations, provision for education, against illness, etc. While much more systematic research should be done before a full comparative analysis of this kind could be made, even these illustrations and many others available seem to support, in a general way, our hypothesis.

#### *Ritualized Personal Relations as Mechanisms of Social Control*

We have seen, then, that the various types of ritualized personal relations serve as mechanisms of social control and mitigate some types of tensions and strains which are inherent in the structure of some types of predominantly particularistic societies. Their ability to perform these functions of social control and alleviation of tensions that arise under the conditions specified above is made possible because the very nature of these relationships is set firmly within the basic structural principles of these societies. As has been pointed out above, they are particularistic, diffuse, and are sanctioned in terms of the most important values and symbols of their respective societies. Thus, they do not go beyond the basic orientations of the society and the consequent expectations of their members, but they rearrange these orientations and the balance between solidary and instrumental relations in such a way as to mitigate the tensions arising out of the particular organizations of these societies. Moreover, it is the fact that they are sanctioned by the highest ritual sanctions that enables these relationships to 'compete,' as it were, with the usual kinship, etc., relations which are also related to the ultimate values of these societies (I am indebted to D. Aberle for this point).

In a general way it can be said that these relationships are able to perform some functions of social control in these societies because (a) they are organized within the framework of the basic orientations and values of their respective societies and (b) their organization is asymmetrical and yet complementary in relation to the organization of those areas in the organization of these societies in which most of the strains are generated. This is especially seen in the organization of instrumental and solidary relations on the one hand and of personal and particularistic relations on the other. Thus while in these situations in which the strains are generated the instrumental obligations are set within the framework of ascribed, non-voluntary, solidary groups which limit the area of individual choice, in the ritualized personal relations the instrumental relations are also set within a solidary and ritual framework—but one that is personal and voluntary, and that cuts across existing solidary groups. Thus these relations provide a new type of solidary framework for instrumental relations which is different from that of the main groups of the society and yet complements them. In this way they seem to conform to some general characteristics of mechanisms of social control.<sup>21</sup>

Because of this it can also be understood that these types of relationships do not usually arise or exist under different structural conditions and in different types of societies. The tensions analysed here seem to be peculiar to the type of predominantly particularistic societies discussed here. In other types of societies, many of these peculiar strains are taken care of by other institutional devices. This is especially true of specificity-oriented groups and relations. These may be either particularistic (e.g. guilds) or universalistic (as most modern vocational, economic, etc., organizations). In the first case, the limitation of relations to some specific areas narrows and much more clearly defines the area of mutual obligations. The particularistic specific groups still provide the individual with various benefits of mutual help, etc., but of a much more clearly defined and limited type than in kinship or in 'ritual personal' relations. Foster's analysis shows that in such societies or sectors thereof, ritual kinship relations are very weak, if they exist at all.<sup>22</sup>

In universalistic societies the multiplicity of such specifically oriented groups and organizations, as well as the clearer definition of jural obligations in universalistic terms, also minimizes this type of tension. It has therefore been rightly shown by Foster, Mintz and Wolf, that ritual kinship does not flourish under conditions of modern, formal, political organization and general market conditions.<sup>23</sup>

Moreover, it is well known that when universalistic institutions impinge on societies which have these types of relationships, the entire content and direction may change, the relationships may become weakened, become sources of new tensions, etc. A complete analysis of these phenomena is, however, beyond the province of this paper.

#### *Problems for Further Research*

If the hypothesis presented here on the social conditions under which the various types of institutionalized personal relationships exist and on their function in their respective societies is in general borne out by the data, it does not yet mean that all the problems connected with it are solved. Rather on the basis of this hypothesis, some new and additional areas of research can be indicated in this field—and it is to be hoped that if such research is undertaken it will help to modify and elaborate this hypothesis. First, we need much more systematic data on the distribution of such relationships—wherever they exist—between different individuals and members of different sub-groups of the society. The same applies to the systematic delineation of the institutional spheres in which these relations are most operative, and the relations between these spheres and the specific contents and durability of obligations covered by these relations.

Second is the general relation of this type of relationships to tensions inherent in a particularistic type of society and to other types of social control operative in it. As has been pointed out above, it need not be assumed that whenever such tensions occur and exist, these mechanisms of social control will necessarily arise. It is, therefore, important to analyse the conditions in which they do not arise and the effect of such a situation on the integration of the society.

Neither, as we have seen, are these relations the only type of mechanism of social control which exists in such societies, nor is it the only type of control which need arise under the specific conditions analysed here (although it seems, if our hypothesis is correct, that only such conditions may give rise to this particular type of relationship).

Within each of such societies there exist, as has been shown, many different mechanisms of social control, some of which may be directed also to these specific problems and tensions. It would, therefore, always be profitable to analyse this particular type of social control along with the others existing in a society, and to analyse their interrelationships. Such an analysis would probably throw some light on the differential distribution of this particular type of relationship.

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#### Notes

<sup>1</sup> M. N. Srinivas, 'The Social System of a Mysore Village,' in McKim Marriot (ed.), *Village India*, Chicago, 1955, pp. 28ff.

<sup>2</sup> M. Fried, *Fabric of Chinese Society*, New York, 1953, pp. 206ff.

<sup>3</sup> B. Paul, Ritual Kinship, with Special Reference to Godparenthood in Middle America (Ph.D. Thesis, University of Chicago).

<sup>4</sup> H. Tegnæus, *Blood Brothers*, Stockholm, 1952, and J. Gibbs, 'Institutionalized Friendship as a Social Mechanism: A Case Study of Blood Brothers,' (duplicated) papers of the seminar on Friendship, Professor C. DuBois, Harvard.

<sup>5</sup> S. W. Mintz and E. R. Wolf, 'An Analysis of Ritual Co-parenthood,' *Southwest J. Anthropol.*, Vol. VI (winter, 1950), pp. 341-368; G. M. Foster, 'Cofradia and Compadrazgo in Spain and Spanish America,' *ibid.*, Vol. IX (spring, 1953), pp. 1-28.

<sup>6</sup> Some of the most interesting are:

Adams, L., 'The Social Organization and Customary Law of the Nepalese Tribes,' *Amer. Anthropol.*, Vol. XXXVIII (1936), pp. 533-597.

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For a full bibliography, see Tegnæus, *op. cit.*

<sup>7</sup> This follows the definition used by T. Parsons and E. Shils in *Towards a General Theory of Action*, Harvard, 1952.

<sup>8</sup> This point has been rightly stressed by B. Paul, *op. cit.*

<sup>9</sup> Foster, *op. cit.*; Mintz and Wolf, *op. cit.*

<sup>10</sup> See D. G. Mandelbaum, *op. cit.*

<sup>11</sup> See E. E. Evans-Pritchard, *op. cit.*

<sup>12</sup> See Gibbs, *op. cit.*

<sup>13</sup> This definition is also based on that by Parsons and Shils, *op. cit.* For a fuller distinction between 'particularistic' and 'universalistic' societies see my *From Generation to Generation: Age Groups and the Social Structure*, Glencoe, 1956, Chapter III.

<sup>14</sup> See Herskovits, *op. cit.*; Fürth, *op. cit.*

<sup>15</sup> On this see especially M. Fortes, 'The Structure of Unilineal Descent Groups,' *Amer. Anthropol.*, Vol. LV (1953).

<sup>16</sup> A good illustration of this point can be found in E. Colson, 'Social Control in Plateau Tonga Society,' *Africa*, Vol. XXIII (1953). I do not know, however, whether among the Tonga there exists any type of institutionalized friendship, but they do have a joking relationship.

<sup>17</sup> See, for example, the following:

Bellah, N., *Apache Kinship Systems* (Harvard, 1952) (for a comparative study of interpretative problems in different tribes).

Colson, E., 'Clans and the Joking Relationship among the Plateau Tonga of Northern Rhodesia,' *Kroeber Anthropol. Soc. Papers*, Nos. 8-9 (1953), pp. 45-61.

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<sup>18</sup> M. Herskovits, *Dahomey*, loc. cit., and S. F. Nadel, *op. cit.*

<sup>19</sup> Mintz and Wolf, *op. cit.*, p. 361.

<sup>20</sup> Mintz and Wolf, *op. cit.*

<sup>21</sup> See T. Parsons, *The Social System*, Glencoe (Free Press), 1951; T. Parsons, E. Shils and R. Bales, *Working Papers in the Theory of Action*, Glencoe (Free Press), 1953; S. N. Eisenstadt, 'Studies in Reference Group Behaviour. I,' *Human Relations*, Vol. VII, No. 2.

<sup>22</sup> See Foster, *op. cit.*

<sup>23</sup> *Ibid.*

## SHORTER NOTE

**Examination of 146 South Indian Aborigines for Haemoglobin Variants.** By Dr. H. Lehmann, St. Bartholomew's Hospital, London, and P. K. Sukumaran, Human Variation Unit, Indian Cancer Research Centre, Bombay

97 It is now an accepted fact that some of the aboriginal communities of Southern India possess the sickling gene (Lehmann and Cutbush, 1952; Büchi, 1955; Foy, Brass and Kondi, 1956). The presence of the sickle-cell haemoglobin has been demonstrated by the sickling test. In this procedure the red cells are de-

oxygenated and the reduced sickle-cell haemoglobin being sparingly soluble forms intra-cellular tactoids. These tactoids are responsible for the bizarre shape of the 'sickle' cell. Sickle-cell haemoglobin can also be demonstrated by an electrophoretic analysis of the haemoglobin, and this technique has led to the discovery of other haemoglobins which cannot be identified in any other way.

In India haemoglobin D has been found in Sikhs and Punjabi Hindus (Bird and Lehmann, 1956) and in Gujaratis (Jacob,

(Lehmann and Raper, 1956). Haemoglobin E was first discovered in a child whose father was of part Indian origin, and was later found in that father also (Itano, Bergren and Sturgeon, 1954; Sturgeon, Itano and Bergren, 1955). The Veddas of Ceylon who are related to the Veddoids of Southern India do not possess the sickling gene, but haemoglobin E was found in six of 167 examined (Aksoy, Bird, Lehmann, Mourant, Thein and Wickremasinghe, 1955).

TABLE I. HÆMOGLOBIN IN THE NILGIRIS

Community	No.	Haemoglobin		
		Sickling Test Positive	Normal Adult Haemoglobin Only	Mixture of Normal Adult and Sickled Cell Haemoglobins
Badaga	30	2	28	2
Iruka	18	4	14	4
Kotha	22	0	22	0
Kurumba	26	7	19	7
Toda	50	1	49	1

It was thus of interest to submit to electrophoresis the blood of aborigines from Southern India. Non-related individuals numbering 146 were examined. All samples were tested for sickling, and it was confirmed again that this phenomenon occurs among the Badagas, Irukas, Kurumbas and Todas. In all instances where the sickle-cell test was positive electrophoretic analysis

showed the haemoglobin to be a mixture of sickle-cell and normal adult haemoglobin. No variants of normal adult haemoglobin other than sickle-cell haemoglobin were discovered.

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## REVIEWS

### AFRICA

**The Judicial Progress among the Barotse of Northern Rhodesia.** By Max Gluckman. Manchester (U.P.), 1955. 98 Pp. xxiii, 386, 2 maps. Price £1 17s. 6d.

This book, the first of a trilogy on the Rotse legal system, marks a notable advance in the analysis of primitive legal concepts. It is a sociological exposition of the logical processes implicit in the actual conduct of Lozi trials and is written by an anthropologist who has had a legal training and whose analysis will interest not only anthropologists and sociologists, but also students of jurisprudence. The analysis is based upon case material largely collected by personal observation of trials, skilfully links Lozi and modern legal concepts, and enables the author to re-examine a number of fundamental jurisprudential questions such as the reasonable man, the nature of law and its relation to morality, and the paradox of the certainty of law and the uncertainty of judicial decisions.

A short review can hardly do justice to the ingenuity of the argument, the apt use made of Lozi linguistic categories and the penetrating analysis of case material in terms of serving new and, it seems, fruitful concepts. The main thesis is that Lozi judicial logic and administration of justice differ in no fundamental feature from ours. The same principles govern the task of the judges. The greater emphasis upon reconciliation of the parties is not a point of difference, as many writers have urged, but a reflection of the wider functions which judges unaided by lawyers (to whom our legal system assigns this task) must assume. Lozi judges use the concepts of reasonable and customary in determining how far a litigant's conduct falls short of that required by law and here again, in the absence of counsel, they themselves must undertake the task of cross-examination. The way they proceed, by attacking statements which show deviations from the standards of the reasonable and customary, gives the erroneous impression that the defendant is presumed to be guilty, for the questions are framed to test statements as if they are false, though not necessarily believed to be false.

Lozi distinguish between the requirements of law and morality, and in order to measure the range of permitted departure from the highest standards of duty and minimum adherence insisted upon by law, the courts invoke the concept of the reasonable as a yardstick.

Moral standards are applied to assess behaviour for evidential purposes, but the standard of the reasonable man is used to decide whether a man has complied with legal requirements. Consideration is given to a man's status, since his role, and hence these two norms of conduct, depend upon his status. In addition, in determining what is reasonable and customary, the court incorporates accepted ideas of morality and justice which therefore influence the judicial process; for instance, it may on grounds of equity urge a man to be generous though it cannot force him to do more than his legal duty. But besides being a judicial it is an administrative body: in that capacity it can secure compliance in other ways, such as threatening to depose a headman whose legal title to fish dams is valid but who refuses to be generous enough to allow their use by a kinsman in need.

Lozi judges extract legally enforceable rules from the whole body of law (*corpus juris*) which includes rules accepted by all normal members of society as defining right and reasonable ways in which they ought to behave in relation to each other and to things. Herein they follow definite principles but allow themselves considerable discretion, as they can do because of the flexibility of the standards implied by the concepts of customary and reasonable. Thus the legal rule itself, while remaining unchanged, can be adapted to changed conditions. This certainty of law as contrasted with the uncertainty of judicial decisions has an important social function. Lozi judges can manipulate the flexibility of legal concepts in order to achieve justice, and law as a *corpus juris* remains certain because of the flexible uncertainty of legal concepts. Judicial manipulation is aided by the fact that legal concepts can be arranged in a number of hierarchical categories, each characterized by different ambiguities or flexibilities.

One of the weaknesses of this stimulating analysis of law in action is the repetitiveness of the argument and the illustrations. It could be made clearer and more readable by taking in slack and avoiding reiteration. The different senses in which the word 'reasonable' is used also detract from clarity. Sometimes it is given the usual legal meaning, but this is not the sense of its use in such phrases as a 'reasonable' thief or a 'reasonable' adulterer. That an adulterer likes to make presents to his mistress is conduct we may

expect from such a deviant stereotype, not conduct constituting the minimum adherence insisted upon by law. Similarly the distinction drawn between *legal rules*, i.e. rules extracted from the *corpus juris* by judges and enforced by the courts, and *rules of law* which are to be found in societies without courts, also leads to confusion. The author gives a valuable analysis of multiple relationships and the concern of judges to maintain them by reconciling the parties, instead of merely resolving the issue presented by them, which is the objective in single-interest relationships; valuable also is his suggestion that the reason for the greater stress upon reconciliation in African courts than in ours may be sought in the absence of lawyers in theirs. But the application of these ideas to Lozi divorce cases seems doubtful and, if there are no lawyers, there often are intermediaries in African societies whose task is to negotiate settlements; yet the stress upon reconciliation in the courts remains. Often the author appears to substitute his refined analysis of judicial logic for the Lozi judges' concrete approach, which involves perception of situations and relationships rather than interpretation of abstract conceptions applicable to such situations and relationships.

These are, however, minor details which should not detract from the massive significance and soundness of this contribution to the sociology of African law.

J. D. KRIGE

**Contribution à l'Ethnographie des Kuta, Part I.** By Efraim Andersson. *Studia Ethnographica Upsaliensia VI.* Uppsala, 1953. Pp. xvi, 365, 38 text figs., 2 plates, 1 map. Price £6

**99** This is the first monographic treatment of a large and important Bantu-speaking people of French Equatorial Africa. The Kuta, or Bakota, tribes extend from the bend of the Niari River in the south to the middle Sangha River in the north. They have long represented a large and irksome lacuna in our knowledge of Western Congo cultures. Their literature was previously scattered through sundry publications, some of them difficult of access.

Dr. Andersson's knowledge of the Kuta is the result of direct experience. He has travelled extensively in the lands of the Southern Kuta, among whom he found several valuable informants, one of them of quite advanced age. This background has served him well in the integration of previously published works into the total picture of their culture. He is availed of another important source of information through his access to unpublished documents of Swedish missionaries to the Kuta.

At the outset of the study any intent to write a definitive treatise is disclaimed. The author stresses that his purpose is to contribute to an ethnography of the Kuta, rather than to assume the onus of a complete description. The reason for his reticence appears to be twofold. In the first place his experience and most of his sources are limited to the Southern Kuta tribes. His knowledge of the northern groups is largely dependent upon the writings of A. Even who studied only certain aspects of Kuta groups around Okondja. It seems that one must read this book with the caution that a possibly more sophisticated culture than the typical Kuta is being discussed, one bearing a strong imprint of the neighbouring Teke and Kongo peoples.

More or less recent external influences might be implicit in such Southern Kuta traits as powerful chiefs, social control by secret societies, vertical looms for weaving raphia fibre, rather naturalistic dance masks, and penannular money. The neighbouring Pangwe share a number of Kuta traits but apparently not these, a disparity which might invite speculation as to their age among the Kuta. A better knowledge of Northern Kuta tribes would be essential in determining which culture elements were typical before contact with southern peoples.

The second reason for avoidance of an unqualified ethnographical approach would seem to lie in the author's great concern with the tenets of the *Kulturkreise* school. Description of Kuta culture elements is throughout augmented with copious references to comparable phenomena among other West African tribes, probably to suggest historical and areal patterns. The concluding chapter is devoted to discussing the origin of Kuta traits among the larger culture complexes of Africa. This interpolation of theory into an ethnographical work might tend to lessen facility of reference. The perhaps premature effort to develop a larger background against which to study

the Kuta dissociates cultural aspects which would be more meaningful in an integrated state.

Withal, the author's historical orientation has given much to the purely ethnographical content of his study. A keen interest in past usage seems to have pervaded his inquiries. Wherever possible, the reader is afforded a glimpse of Kuta life before European contact. In its scale, it seems to have been marked by a certain barbaric splendour.

This first volume discusses historical and geographical background, material culture (including commerce), family and social life, and social and political organization. Greatest stress seems placed upon secret societies, especially Mungala, which seems a tribal society, formerly encompassing all Kuta men. In seeking to trace the origin of Mungala much space is devoted to the typology and distribution of secret societies, first in the environs of the Kuta and then in the West African area.

Certain of the author's inquiries bear strongly upon problems already familiar to students of the material culture of the area. Since Du Chaillu's time the bird-headed type of throwing knife has been ascribed to the Pangwe peoples. Dr. Andersson found that this weapon is not only made by the present-day Kuta, but is said by them to be an old element of their culture. He offers several well-taken points in support of a Kuta origin for the form, but somehow overlooks the argument through visual evidence: the weapon in the shape of its blade and the treatment of its handle is much more closely related to typical Kuta short swords than to any Pangwe arm.

The metal-covered wooden funerary figures so characteristic of the Kuta have been thought to function as guardians of baskets of skulls in the ancestor cult. This opinion, probably derived from the extension of an analogy with Pangwe funerary figures, finds no support in this volume. The role of these figures remains unclear, as does their significance; the author suggests that a 'god of the dead' may be indicated. Nor could native informants clearly explain the peculiar abstract aspect of these figures; this vagueness could suggest considerable age for the conception.

One of the most striking elements in these figures is a crescentic form over the face. The author mentions several things which this might represent: a horizontalized sagittal crest, the new moon, the rainbow, or the tutelary spirit of the Mungala society, a mythical beast embodied in a large mask structure of bicornous shape. However, no conclusion is reached as to the symbolism, simple or composite, of this motive. Another crucial question concerning these images is completely overlooked: why are they covered with metal?

Certain of the questions posed by the first volume may be answered in the second, which is to discuss religion and other aspects of culture. The text of this work mentions certain items, such as a type of chief's chair, a wicker shield, and a woven cap worn to protect the hairdress, which the reader might wish to compare with types made by other tribes. As these cannot be visualized from the casual verbal treatment they receive, fuller, or better chosen, illustrative material sometimes proves desirable. The articles which are illustrated are very capably rendered, although several are not from the Kuta, but nearby and culturally distinct peoples. When one finds that musical instruments firmly attributed to the Kuta are almost completely lacking in the literature and in museum collections, their absence so far in a book devoted to this people is disappointing. In the next volume a glossary of Kuta terms most frequently used in the text would be most helpful.

However specialists may regret the neglect of their problems, there can be no detracting from the great worth of this book. Dr. Andersson succeeds in acquainting us well with a culture hitherto obscure. This first volume should at the very least serve as a point of departure for many inquiries into special aspects of Kuta life.

LEON SIROTO

**Le Culte des Saints dans l'Islam maghrébin.** By Emile Dermengham. Paris (Gallimard), 1954. Pp. 351. Price 990 francs

**100** This interesting study of the cult of saints in contemporary North African Muslim society will be welcomed, more particularly by ethnographers and folklorists. It is based on some ten years' close observation and detailed recording in many parts of Barbary, and thus makes reference to other parts of

the Muslim world only for purposes of comparison. The work is arranged in five parts, concerned with general attributes of the numerous saints and associated holy places, with the lives of some selected influential saints, with sanctuaries and rites for individual observances, with large-scale festivals and pilgrimages in connexion with certain saints' tombs, both urban and rural, and with saintly brotherhoods. Well illustrated by reproductions on art paper of

**Proceedings of the Thirtieth International Congress of Americanists.** Held at Cambridge, 18–23 August, 1952. Edited by Mrs. L. E. Joyce, H. J. Brauholtz, G. H. S. Bushnell, Adrian Digby, M. Fortes. London (Roy. Anthropol. Inst.), 1955. Pp. xxx, 249, 66 illustr.

Until this Thirtieth Congress, the International Congress of Americanists had not taken place in England since 1912. It is a sad reflection on the richness of our collections and the eminence of our past scholars that Americanist studies are now so little pursued in this country. Except for a very few people, little use is made of the rich materials available for research, and opportunities for research are rare. It is high time in this present day that full academic backing was given in England to Americanist studies.

The International Congress of Americanists provides a unique opportunity for Americanists to meet and confer. The widespread interest in American studies is manifest by the fact that 21 countries sent official delegates to this Thirtieth Congress at Cambridge. 65 papers were read; 39 are published in this volume together with six abstracts; the full text of the majority of papers is included; the papers are in English, French, Spanish or German.

Conferences generally produce a miscellany of papers, some of wide interest, others of interest to but a few. An indication of the content of this volume would seem to be of most interest to the reader of this review, for the variety of topics covered and the regions with which the papers are concerned is considerable. From the point of view of regional representation, the volume contains for Alaska—2 papers, Argentina—3, Brazil—5, Canada—1, Columbia—1, Cuba—1, Ecuador—1, Maya area—7, Mexico—2, Middle America—1, Peru—7, U.S.A.—4, North America, general—1, South America, general—3, Americas, general—5.

In Mayan studies, Ruiz summarizes four seasons' (1949–1952) archaeological discoveries of architecture, sculpture, painting and ceramics at Palenque. Important and fine additions in sculpture to the already rich material in bas-relief were made, but of these outstanding finds perhaps the most striking is the large altar of the crypt beneath the Temple of Inscriptions, claimed as one of the most important examples of Mayan art. Mayan epigraphy has also been greatly enriched with the discovery of new inscriptions. A. H. Anderson draws attention to the significance of sites found in British Honduras for Mayan research. Thompson speculates interestingly on the character of the Maya today and in pre-hispanic times, the bearing this character had on the outstanding aspects of Maya culture, and particularly its relation with the overwhelming Mayan preoccupation with time. In the decipherment of Maya glyphs considerable progress has been made in recent years with particular attention to minor elements. An important step forward was Thompson's contribution that such elements could sometimes express syllables. Barthel's paper discusses his recent work on affixes to Maya glyphs. Two papers are concerned with the Maya—Quiché: Gerard discusses their origins, Spence considers the folklore of *The Popol Vuh* and maintains 'that its separate incidents were first composed in Mayan verse, and that these verses were handed down traditionally by word of mouth and were later woven into one continuous saga by the skilful hand of a native poet of address and craftsmanlike ability' (p. 53).

In the field of prehistory, Bushnell discusses the stone carvings of Manabi, Ecuador. On Peru, Rossel Castro writes on the geometrical figures associated with Nasca culture found in the Hoya de Rio Grande. Kosok maintains that the Inca roads were not built for commerce but for military and political reasons. As the power of the Chimu Empire on the coast grew, and similarly and subsequently that of the Inca Empire, so did road and canal building increase.

many photographs of saints' tombs and of other holy places, the work also has adequate footnote annotation throughout, although it lacks a general bibliography.

It forms a useful contribution to the study of the persistence of pre-Islamic indigenous religious and social features with their subsequent Muslim accretions, in the distinctive contemporary Muslim world of Morocco, Algeria, and Tunisia. WALTER FOGG

## AMERICA

These forms of communication were a natural concomitant of the ascension to and maintenance of political control of a few, of the ruling Inca classes, culminating in the north-south road system of the Incas that ran from southern Columbia to central Chile. For the coast, Kosok gives five types of road construction found. For Brazil, Castro Faria discusses the problem of the natural or artificial origin of *sambaquis*, which was first introduced into the literature in 1885 by de Lacerda's study and on which the bibliography has grown considerably though it does not include one complete study. Castro Faria reviews briefly his findings on excavating the *sambaqui de Cabeguda*, Laguna, Santa Catarina, and shows that these burials cannot possibly have a natural origin. On the Argentine, Pedersen writes on bronze objects found in the Chaco-Santiagüena region which, he concludes, formed part of the area of Andean expansion; Márquez-Miranda's paper is on finds made in the Quebrada de Humahuaca in the state of Jujuy; Donner contributed a paper on the collections of figurines of the Diaguita area in the Vienna Ethnographic Museum.

The reconstruction of a culture or aspects of it frequently means working with what is distinguished as the archaeological record on the one hand and the historical on the other. The passage from pre-hispanic times to the chronicled sources of the sixteenth and subsequent centuries, in Americanist studies, is often afforded by a few surviving codices and documents; the early post-Conquest Spanish commentaries help the reconstruction of the indigenous cultures in this protohistorical period. Yeoman in a paper on musical instruments of pre-Columbian Central America and Burland on the Toltec calendar have recourse to this type of evidence, and likewise Heyerdahl in discussing the nature of Peruvian craft for water commerce.

Ballesteros-Gabrois writes of the important discovery in 1951 of the complete manuscript of Fray Martín de Murua's *Historia General del Perú* (1611) in the Duke of Wellington's library. Other papers in history are on the work of Maximilian of Wied, who travelled extensively in Brazil in 1815–17 and in North America in 1832–34, on Taino Indian remnants in south-eastern Cuba and on the *encomienda* at Potosí, Peru. There were some 25,000 displaced Indians doing forced labour in the silver mines at Potosí in 1549 when a *cédula* ordered their repatriation; most refused to return saying that they were better off at Potosí than in their homes. If the exploitation of the silver of Potosí had been abandoned much of the prosperity deriving from Peru would have been at stake. Hence the *encomienda* remained the system of indigenous working of the Potosí mines until its replacement by enforced Indian labour under the Viceroy Francisco de Toledo.

In North American ethnology, Cartwright discusses variations in American Indian beadwork designs and possible reasons for these variations. During the early period of the introduction of glass beads from Europe, designs of the simple, geometric linear kind employed prehistorically in porcupine-quill work were continued in the new medium. This art form covered most of the forested area of North America north of 40° N. and south in the Rocky Mountains. Subsequently, due to various influences, this region broke up into three design areas: in the east, delicate geometric scroll and floral work designs; in the Great Lakes area, heavy floral work, and in the west, geometric designs. In a second paper, Cartwright, comparing traits of a Washo girl's puberty ceremony with that among the Californian tribes, Paiute and Shoshone, speculates that the Washo were in the past a Californian tribe. Underhill describes a modern version of the use of *peyote* among the Cheyenne-Arapaho. Rousseau reconstructs the religion of the Montagnais and Huron as it was before Christianization. By fieldwork and comparison with early

historical records he confirms that the Huron have retained no trace of their religion prior to French contact, whereas Montagnais ritual seems largely to have survived, their mythology and cosmology, on the other hand, having changed.

In South American ethnology, Caspar describes the penis cover of the Tupari and related behaviour associated with the protection of sexual organs among Tupari men and women. Zerries maintains that outside the high cultures of the Andes, magico-religious life in South America centres on the figure of the medicine-man. He reviews the spirit beings with which the medicine-man chiefly associates and concludes that basically his primary role is in the sphere of hunting. Where the shaman's ritual functions are agrarian, Zerries feels that hunting ceremonies have been extended to the vegetable world. Schaden, writing of the migrations of the Guarani as due to their holding to the idea of a Paradise to be sought across the sea, discusses the effects of this myth on the present-day *Weltanschauung* of the Guarani. Baldus speculates on the reasons for a hunter not eating the game he kills, a custom reported as exclusive to the Puri of Minas Gerais, the Kaingang of São Paulo, the Botocudos of Santa Catarina, the Kraho of Goyaz, in Brazil and to the Siriono of eastern Bolivia. Friede presents a study of acculturation of the Kofan based on historical documents and fieldwork. The Kofan, a tribe which, together with the Sucumbio, Andaki and

Tama, forms an enclave between the Andean tribes proper and those of Amazonia in the frontier region of Columbia with Ecuador, exemplify well the progressive depopulation of the Selva that has been going on since the Conquest: in the sixteenth century they were calculated as consisting of 15,000 taxpayers and are now reduced to three hundred individuals.

In linguistics, Hammerich contributes a paper on the dialect of Nunivak, a good example of South-Western Eskimo, and another on Russian loan words in Alaska. Reyniers gives texts on nicknames in Peru.

On the origins of New World culture, Heyerdahl contributes two further papers: in the one, he refers to the objectives and results of the famous Kon-Tiki expedition which refuted previous contentions to the contrary by demonstrating that Peruvian balsa craft were thoroughly sea-worthy and capable of deep-sea voyaging; in another paper he pleads that aboriginal America cannot be left out in any serious discussion of Polynesia, that happy hunting ground of theories of origin and movements of people. Haudricourt refutes Anderson and Stonor's recent claim that maize originated in the mountainous regions of Indo-China. Even if certain varieties of maize from Assam are only found in central Peru and nowhere else that does not constitute proof of relations between these two regions before the sixteenth century.

PHILIP DARK

## CORRESPONDENCE

**The Lough Erne Sculptures.** Cf. MAN, 1953, 53; 1954, 125. With a text figure

**102** SIR.—I wish to answer briefly Mr. Lethbridge's paper 'Christian Saints or Pagan Gods?' which recently appeared in the *Journ. Roy. Anthropol. Inst.*, Vol. LXXXIII, pp. 175ff. In this article he contests the view advanced in my paper (MAN, 1953, 53) that three of the stone figures on White Island represent St. Patrick, King Leiguire and his son Enna. At the same time I wish to present some further evidence, unknown to me three years ago, in support of my theory. I can deal here only with the largest figure which in my opinion represents St. Patrick and not a pagan king or chieftain 'stroking his chin in thought.'

1. In 1954, an Irish crozier-head was discovered near Stockholm. In his detailed appreciation, Mr. W. Holmqvist came to the conclusion that it 'differs both in size and form from all the other [extant] croziers' (*Antiq. Journ.*, Vol. XXXV, p. 51). After this find it is, of course, not possible to ascertain that no other types of Irish croziers ever existed.

Mr. Lethbridge's drawings, derived from photographs, contain certain inaccuracies. For instance, he depicts the lower part of the crozier as a rather thin and indistinct feature, whereas in fact its shape is clearly defined and rather prominent. In this connexion the following passage from the 'Annals of Tigernach' (*Rev. Celtique*, Vol. XVII, p. 383) is worthy of note: the bell was struck against the king of Teffa 'with the end of . . . the "staff of Jesus,"' i.e. the most famous crozier, said to have been given by Christ Himself to St. Patrick. If 'the staff of Jesus' and the crozier held by the White Island figure resembled the Stockholm crozier, this quotation would be more significant than O'Donovan's remark quoted in my paper (*op. cit.*, paragraph 9).

The head of a short sceptre-like crozier, very similar to that held by the White Island figure, is actually represented on the Cross of Clogher (A.D. 1300-1350), now in St. MacCarten's Seminary, Monaghan (J. Raftery, *Christian Art in Ancient Ireland*, Vol. II, p. 163, plate 110). The Revd. J. E. MacKenna identified the saint, holding this 'pastoral staff,' as St. Patrick (*Ulster Journ. Archæol.*, 2nd ser., Vol. VII, pp. 113ff.). He further pointed out that two Clogher Saints, both distinguished metal workers, were closely associated with Lough Erne.

2. With regard to the tonsure of the largest White Island figure, Miss M. A. Bennet-Clark (MAN, 1954, 125) asks why St. Patrick 'should have been singled out from the rest of the similarly tonsured Celtic clergy' by his nickname 'adzehead'? Neither in written records, nor in sculpture, was St. Patrick thus singled out. In the *Tripartite Life* the term 'adzehead' is used for clerics in general

(p. 164) and I have found yet another representation of the Celtic tonsure of the shape evidenced by the White Island figure (fig. 1). It belongs to a head, probably the portrait of a cleric, carved on a small Latin cross of granite. The cross, formerly in the main street of Blackrock, Co. Dublin, was shown to me in July, 1954, at the Corporation Store at Dun Laoghaire. Mr. T. Mason, of Dublin, kindly informed me that the Blackrock Cross, quite near to



FIG. 1. THE BLACKROCK CROSS

Monkstown, was an important boundary stone. 'In olden days the Mayor of Dublin used to ride once a year to the stone cross; from there he turned towards the sea, into which he flung a dart, claiming that Dublin's jurisdiction would extend to where the dart dropped.'

As early as 1860, G. V. Du Noyer ('Remarks on ancient effigies . . . on White Island,' *Journ. Kilkenny Archæol. Soc.*, New ser.,

Vol. III, pp. 64ff.) quoted a great wealth of ancient texts in order to prove that the tonsure 'from ear to ear' was the hairstyle worn by the earliest Celtic monks and ecclesiastics. The tonsure of the Blackrock Cross lends further strength to his view.

3. Mr. Lethbridge calls the monuments under discussion 'the Lough Erne figures.' As there are other important sculptures on the shores of Lough Erne, I suggest that the name 'White Island figures' be retained.

ELLEN ETTLINGER

**IO3** Str.—In 1953 I wrote an article about these figures on White Island, Lough Erne, which has recently been published in the Journal (*J. R. Anthropol. Inst.*, Vol. LXXXIII, Part II, pp. 175ff.). During those three years more information has accumulated which, judging by the numerous letters I have received on the subject, will be of interest to readers of MAN.

First, it is, I think, clear that I was wrong in suggesting that Sheila-na-gigs were an introduction of Norman date, although there appears to be evidence pointing to a resurgence of paganism at that time. Dr. A. E. Mourant most kindly wrote to inform me of a reference to a real Sheila-na-gig from White Island itself (George Ryley Scott, *Phallus Worship*, London (Torchstream Books), p. 241). The carving was apparently at one time in Dublin Museum and may still be there. It seems to be in the same style as the other cross-legged female figure from Lough Erne (*J. R. Anthropol. Inst.*, *ibid.*, fig. 1 d) but is obscene. This gives us a sixth figure from the site and tends to confirm my suggestion that there were once several more of these standing stones.

The second point is of equal interest. In *The Dark Ages*, edited by D. B. Harden (London (Methuen), 1936), Professor I. A. Richmond, in an article entitled 'Two Celtic Heads in Stone from Corbridge, Northumberland,' publishes carved heads which he identifies with Maponus. These have the same socket or hollow in the crown of the head as is found at Lough Erne and this is identified by Professor Richmond as a *locus* for offerings. The better carved of his two heads has the same thoughtful expression and general appearance as the Lough Erne figure, which I tentatively suggested might be identified as a king or chieftain (*ibid.*, fig. 1 a). I learn from Professor Richmond's article that Miss F. Henry (*Irish Art in the Early Christian Period*, London, 1940) had already noted the resemblance between the Corbridge head and that on the Micklebostad bowl (*J. R. Anthropol. Inst.*, *ibid.*, fig. 1 g). Further than this, she has identified two Janus-headed Irish stone carvings at Boa Island, Co. Fermanagh, and Kilnaboy, Co. Clare. Now the second Maponus head from Corbridge is also double-faced. On the one side he is grim and thoughtful; on the other a much younger, moustached man. It seems reasonable then to conclude that all the figures on the Irish buckets and bowls so far found in the Viking graves in Norway, represent either Maponus, whom the Romans equated with Apollo, or the cross-legged lady who I suggest may be Brigid. It is not easy to decide whether the sceptre-like object in the Loch Erne Maponus figure may not have been once part of a harp. Maponus, however, was a harper, as well as a deity of fruits and healing. He may well be pondering on what tune to play.

If this identification is right as far as it goes, then I feel that it is most probable that the seated figure (*J. R. Anthropol. Inst.*, *ibid.*, fig. 1 e) was indeed Cromm Cruach, the high god of all Ireland, with his face injured by the traditional blow from St. Patrick's staff and the roll of sacrificial victims on his knees.

The whole White Island question needs further investigation. Was there once a pagan temple there comparable to the Gaulish shrine of Roquepertuse (Paul Jacobsthal, *Early Celtic Art*, plates 1-4)? We have now six known figures of gods from the one site and there may have been thirteen. Is the figure with drawn sword and shield the war god, Neit, and was the Sheila-na-gig his wife, Nemnon? The grinning old woman may have been the Cailleach. Who was the shepherd god? The difficulty is increased by the variety of names formerly applied to each god. In one tribe the names were entirely different from those used for the same god by their neighbours. Thus, while Maponus may have been the name used by the Brigantes in northern England and Mabon the name of the same god in Wales, he was Diarmid in Scotland and in parts of

Ireland. Ptolemy places the Irish Brigantes further south than Lough Erne and so he was probably not known as Maponus there at all. The subject is difficult, but there is an opportunity here for a considerable advance in knowledge.

T. C. LETHBRIDGE

#### Bilingualism among the Mahass. Cf. MAN, 1955, 164

**IO4** Sir,—Dr. Griffiths' note on 'Bilingualism among the Mahass' makes a number of interesting points. Since we have both frequently lived and worked in Nubia during the last nine years and have employed Nubian servants for the whole of that time a few further comments may be useful.

First, we should like to make the point that the people of whom Dr. Griffiths writes are more properly known as Nubians. Barabra is a term used in Egypt in a disparaging sense (it is presumably connected with barbarian) and is never used by the people themselves nor is it used by anyone in the Sudan. The people know themselves as Mahass, Dongolawi, or Kanuz, according to their section of the Nubian people.

Whatever the position may have been in 1936-37, there is no doubt that nearly all adult Nubians now speak Arabic fluently although they have in no way lost their own language. Our experience both on excavations and in travelling is that only the smallest boys, those still under the influence of the women in the home, have no knowledge of Arabic. The Arabic is, perhaps, not such as a purist would accept and has a number of peculiarities, such as the tendency to add the sound -y to the end of many words; but it is readily understood by monolingual Arabic speakers of the Sudan. Equally, the Nubian has no difficulty in understanding the Arab.

One of us has had the opportunity of meeting and trying to speak with Nubian women. They normally do not speak Arabic at all, except for the younger girls who, of recent years, are learning it at school. Even the womenfolk of such a notable figure as the Omda of Abri were unable to converse in Arabic. There is no question but that Nubian remains the language of the home.

Religion and education have no doubt played an important part in increasing knowledge of Arabic, but one of the main reasons for the acquiring of Arabic by the men, apart from travel, is the necessity of speaking it in dealings with authority. There may be an occasional Government official of local origin who speaks the *rutana* (Arabic for a foreign language, and always used for the Nubian languages when speaking Arabic), but in general officials would speak only Arabic and English.

We doubt whether Nubian will easily die out, or that it is disparaged. We have always found the Nubians proud of their language even though unaware of its long history, and tenaciously holding on to it even when living among Arabic-speakers. The Nubian servants of Khartoum, although many of them are fully literate in Arabic, prefer to speak to each other in their native tongue. We have found that the conversation in our own yard of an evening has always been in the *rutana* except when friends speaking only Arabic have called. In such cases the conversation is apt to change from Arabic to Nubian and back again with no effort and with equal fluency in both languages.

It appears to us that without the intervention of other factors Nubian is likely to remain a live language of the Nubian household with a completely bilingual male population. What is likely to cause its disappearance is the building of the high Aswan dam which by its flooding of a very large part of Nubia, including most of Mahass and Sukkot, will displace the population. This may well cause the death of a language of high antiquity and honourable traditions, with a consequent impoverishment of the Sudan.

PETER AND MARGARET SHINNIE

#### Land Rights in Two Brethren Hamlets: A Correction. Cf. MAN, 1956, 50

**IO5** It is much regretted that, owing to an editorial oversight, the first line of paragraph 7 of this article was printed as: "All the farm buildings extant in 1851 . . ." It should have read: "All the farm holdings extant in 1851 . . ." Owing to the author's departure for Australia, the Hon. Editor received his corrected proof too late to make the alteration in press.





(a) WEOBLEY, HEREFORDSHIRE



(b) PUTLEY LACON, HEREFORDSHIRE

*Photographs: (a), (b), (d) F. C. Morgan; (c) Peter Smith, by permission of H.M. Stationery Office*



(c) TYDDYN CYNNAL, LLECHWEDD, NEAR CONWAY



(d) COURT HOUSE FARM, AYLTON

### THE CRUCK TRUSS

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## Greetings

FROM THE ROYAL ANTHROPOLOGICAL INSTITUTE

**IO6** *The Honorary Editor of MAN has the honour of conveying the warm greetings of the President, Council and Fellows of the Royal Anthropological Institute to the President, Officers and members of the International Congress of Anthropological and Ethnological Sciences, holding its fifth session at Philadelphia from 2 to 9 September. The Institute offers its best wishes for the complete success of the Philadelphia meeting, and these will be conveyed in person by its appointed delegates. The Institute has been closely associated with the Congress from its first beginnings when the late Sir John Myres took the steps which led to the inaugural meeting in 1934; its profound interest in the Congress remains unabated and its only regret is that the attendance from Great Britain cannot be much larger than it is.*

But although the Institute is based in Britain, its Fellowship is drawn not only from the Commonwealth but from all parts of the world; and in the interests of further development of the Institute's international aspect—which is so closely in accord with the objects of the Congress—the Council cordially invites those members of the Congress who would like to know more of the Institute's activities to make themselves known to its delegates, who will be glad to give information, and to act as sponsors for any who may wish to apply for Fellowship. As a gesture of goodwill, the Council has resolved to waive the normal entrance fee of one guinea in the case of all such Congress members who, having applied to the Institute by 31 December, 1956, are elected to Fellowship.

The Honorary Editor takes the opportunity of emphasizing that MAN is at the service of all anthropology and all anthropologists, in the widest sense. He hopes to publish reports on the Congress activities and reminds readers that, by virtue of its monthly appearance, the correspondence columns of MAN furnish an effective medium for the airing, exacerbation and occasional solution of the controversies of the moment.

## THE CRUCK TRUSS\*

by

LORD RAGLAN

**IO7** A 'cruck truss' is the name given to a roof truss composed of two curved or angled pieces of timber, called 'crucks' or 'cruck blades,' set usually on a low stone plinth about 20 feet apart, or sometimes less, with the convex side outward, and joined at the top in such a way as to support the ridgepiece (fig. 1). In all but the crudest forms they also directly support the purlins. Three or more of such trusses, set from 10 to 25 feet apart, form the framework for a house or barn open to the roof. It seems at least that the earlier cruck buildings, up to about 1550, were always open to the roof. After that crucks continued to be used in gables and partitions, and in barns, up to the eighteenth century.

The history of cruck building is obscure. There are references to crucks (variously spelled) in mediæval documents, and other terms, English and Latin, were used which have been supposed to denote crucks. It may be, however, that they denote forked posts, a very widespread

device for supporting roofs. The word 'cruck' is probably akin to 'crook' and 'crutch,' and could be applied to any curved, angled or forked piece of timber.

There are several theories of the origin of cruck building. One is that it is a development from the ancient practice of bending withies or young trees so as to make semicircular huts, or, more elaborately, such rectangular houses as that figured on p. 149 of Professor Grahame Clark's *Prehistoric Europe: The Economic Basis*. But there is little in common between the necessarily slender saplings used in such construction and the massive timbers of which crucks are formed. Flexibility is the essential characteristic of the former, and extreme rigidity of the latter.

A more popular theory has an interesting history. When, about 60 years ago, S. O. Addy was writing his *Evolution of the English House*,<sup>1</sup> his attention was drawn to a house near Scrivelsby in Lincolnshire known as Teapot Hall. This, though of two storeys, was completely triangular in section, but it was built of sawn deal with fittings all of the

\* With Plate H and six text figures

nineteenth century, and was undoubtedly built at some time within that century, presumably as a joke (fig. 2). Addy persuaded himself that it was an old structure with a later fireplace, etc., and that it represented the earliest form of the cruck house. He called the timbers of which it

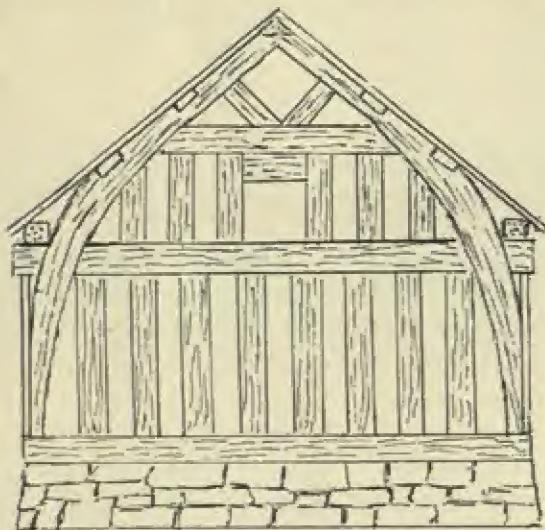


FIG. 1. CRUCK GABLE OF A HOUSE OF C. 1525  
After Fox and Raglan, Monmouthshire Houses, Part I, p. 53

was built (it was destroyed in 1944) 'straight crucks,' though this is a contradiction in terms.

He was followed by Innocent,<sup>2</sup> who says that Teapot Hall 'can hardly be later than the end of the Middle Ages' and regards its type as the link between the triangular huts

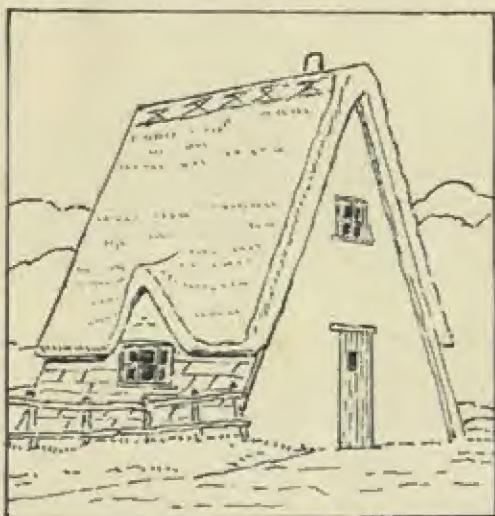


FIG. 2. TEAPOT HALL  
After Addy, p. 42

found in many parts of Europe and the cruck house. He describes the latter as 'peculiarly British.'

Dr. Peate<sup>3</sup> says that 'cruck technique is a feature of building construction throughout north-west Europe,' and cites Erixon as his authority. When, however, we turn to the latter's paper,<sup>4</sup> we find that he bases his case on Innocent and the original 'straight cruck.' We are back, that is to

say, at Teapot Hall. To this Braun<sup>5</sup> similarly directs our attention. Finally Teapot Hall occupies a high place in Mr. James Walton's<sup>6</sup> hypothetical pedigree of the cruck truss.

In fact there are many buildings certainly older than Teapot Hall with straight or nearly straight crucks, and it has been supposed that these are all early. A more careful study of them, however, suggests that they, and all buildings with rough or irregular crucks, are late and degenerate (fig. 3). This conclusion is based partly on the fact that,

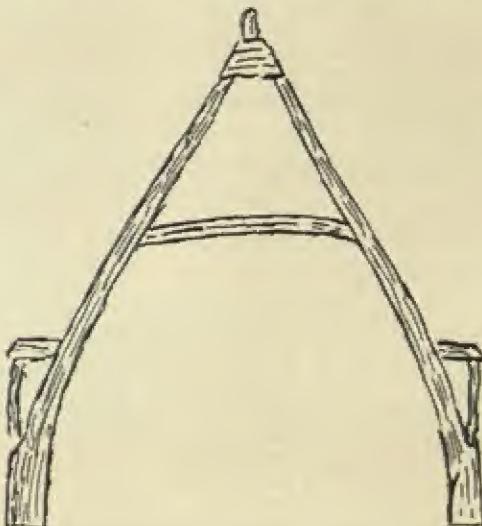


FIG. 3. FRAMEWORK OF LATE (? C. 1700) COTTAGE AT DIDBROOK, GLOS.

being poorly built, they are unlikely to have stood for many centuries, and partly on their associated timberwork and masonry, which do not suggest a remote date. Furthermore, in a fairly large cruck-built house near Leicester which Sir Cyril Fox and I recently examined, we were satisfied that its rough crucks were later than an incomplete truss of good quality which had survived in a wing.

The oldest cruck building which I have seen (or heard of) is the magnificent barn at Great Coxwell in Berkshire. This barn, Mr. W. A. Pantin tells me, may date from the beginning of the fourteenth century. Alternate trusses are of angled crucks set high in the wall. The crucks are of fine quality and the whole technique is highly sophisticated. Its derivation from a 'straight cruck' is almost inconceivable.

It seems likely that crucks were first employed internally, and an open cruck truss, one that is to say with nothing between the crucks except a collar high up, bears a strong resemblance to a Gothic arch. Addy, Innocent and Braun<sup>7</sup> all note this resemblance, but it occurs to none of them that this may be the clue, that is to say that a cruck truss may be an imitation in timber of a stone arch. There is nothing *a priori* improbable in this. We know that stone lintels, mullions and jambs, with their carvings and mouldings, were copied in wood. It seems doubtful, in fact, whether, in Europe at any rate, timber was ever carved or moulded except in imitation of stone. It is not surprising

then to find in a Monmouthshire manor house, probably of the late fifteenth century, an open cruck truss with a Gothic moulding on the crucks (fig. 4), and the fact that Innocent figures a very similar truss from Yorkshire.<sup>8</sup>



FIG. 4. GOTHIC MOULDING ON A CRUCK IN A HOUSE  
OF C. 1480

The cruck is 5 inches thick and 1 foot 10 inches broad.  
After Fox and Raglan, op. cit., p. 32

suggests that the builders were following a fashion widespread among the well-to-do. That cruck trusses were often used for their decorative effect is pretty certain.

The Gothic arch came into general use about 1200, and my suggestion is that in the thirteenth century the cruck truss was devised by sophisticated builders as a copy in wood of, and an alternative to, that arch. At first finely

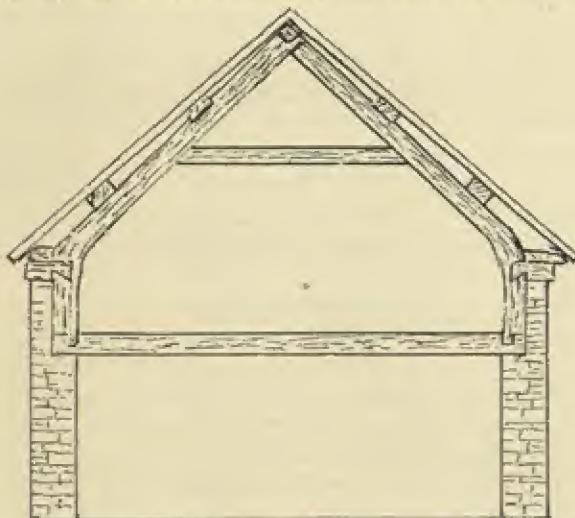


FIG. 5. SECTION OF A STONE HOUSE OF C. 1575 WITH  
UPPER-CRUCK CONSTRUCTION  
After Fox and Raglan, op. cit., p. 70

made and used only in expensive buildings, crucks spread gradually downwards, losing quality as they did so, till they ended in the crudely built cottages which some theorists have supposed to be primitive. Before this happened two variant forms were developed, the upper

cruck and the false cruck. The upper cruck (fig. 5), instead of resting on a plinth, was tenoned into a ceiling beam. The false cruck (fig. 6) is found chiefly in Devon and Somerset. Two pieces of timber are scarf'd together so as

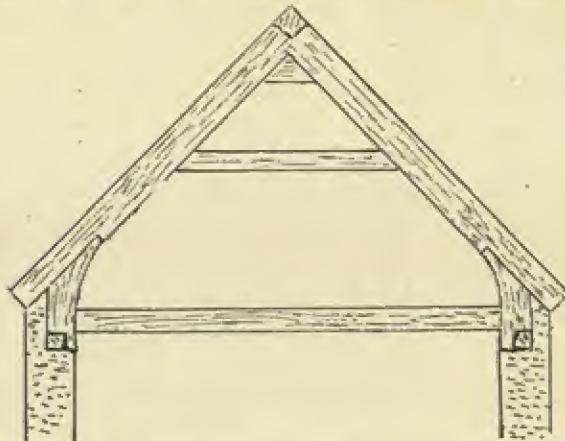


FIG. 6. FALSE CRUCK FROM A HOUSE NEAR EXETER  
The walls are of cob. From a drawing by Sir Cyril Fox

to look like a cruck. This suggests the late spread of the fashion for crucks to areas where there was no suitable timber.

It is of course possible that crucks were older, but the evidence for them before 1200 seems very slight. Mr. F. H. Crossley,<sup>9</sup> indeed, says that 'the early [Saxon] square-ended chapels constructed of timber were designed with a pair of "crucks" or bent timbers, joined at the top and forming a rough arch at either end, united by a ridge pole, the walls of wattle and the roof thatched with reeds.' But he gives no authority for this statement. Other writers rely on the supposed 'primitiveness' of the technique, but this of course is to beg the question. If it were really primitive one would expect a distribution outside Britain, and this, in spite of Erixon, has yet to be established.

#### Notes

<sup>1</sup> S. O. Addy, *The Evolution of the English House*, London, 1933 (first published 1898), p. 41.

<sup>2</sup> C. F. Innocent, *The Development of English Building Construction*, London, 1916, pp. 27, 59.

<sup>3</sup> I. C. Peate, *The Welsh House*, London, 1940, p. 166.

<sup>4</sup> S. Erixon in *Folklore*, 1937, p. 141.

<sup>5</sup> H. Braun, *The Story of the English House*, London, 1940, p. 28.

<sup>6</sup> J. Walton in *Antiquity*, 1948, p. 185.

<sup>7</sup> Addy, op. cit., p. 52; Innocent, op. cit., p. 32; Braun, op. cit., p. 28.

<sup>8</sup> Op. cit., p. 53.

<sup>9</sup> F. H. Crossley, *Timber Building in England*, London, 1951, p. 10.

## THE INTERPRETATION OF PAKOT VISUAL ART by

DR. HAROLD K. SCHNEIDER

Department of Anthropology and Sociology, Lawrence College, Wisconsin

108 Anthropologists seem agreed that aesthetic sense is universal<sup>1</sup> but most would probably agree that standards relating to what is aesthetically pleasing vary

from culture to culture. Nevertheless, in practice scholars who discuss the art of non-literate people do sometimes seem to impute standards to them or, what amounts to the

same thing, try to deduce the standards of beauty of a people by analysis of objects from their cultures. In both cases standards of beauty learned in Western cultures are used as a basis for judging what is or is not art in a non-literate group. Almost all discussions of the Magdalenian people make reference to the 'art' of these cave dwellers.<sup>2</sup> In effect this is the attribution of standards of beauty to Magdalenian people based upon the assumption that their standards were the same as ours. In the study of prehistoric cultures such deductions are inevitable and in fact probably close to the truth, but we can never know for sure.

In the study of contemporary people such deductions are also seemingly common. Since discussions of art seldom include any but an implied note of the standards of beauty of the subjects, it seems possible that what constitutes the art of the people is derived at least in part by deduction.

The present paper is an addition to the limited number of studies of concepts of beauty of non-literate people. It is proposed to show what Pakot<sup>3</sup> standards are and to define their visual art in terms of them. It is further proposed to illustrate from this how deduction of art may lead to erroneous conclusions if the standards of beauty of a people are not taken into account.

The Pakot distinguish between what is useful in subsistence or the ordinary acts of getting a living and what is an aesthetically pleasing embellishment having no subsistence or utilitarian use except as decoration. In this discussion the term utilitarian may be most conveniently defined as anything which has no aesthetic component. Thus the utilitarian object is one that has any function in living other than an aesthetic function. This distinction became apparent during a discussion of a carved wooden milk pot (*aleput*) which has a projecting lip carved into the rim. Informants said that the pot was *karam*, a word usually translated as 'good,' and which may be used in a wide variety of situations. When asked to explain further what was meant by 'good,' one informant said that the pot was useful for holding milk and so was 'good to have.' This informant further stated, however, that the lip of the jug was *pachigh*, a word which had been previously translated by the interpreter as 'pretty' or 'beautiful' and which, it was explained on this occasion, meant 'pleasant to look at' and 'unusual.' Additional questioning elicited the information that the lip was a recent invention by some unknown inventor, before whose time milk pots had had no lips. The lip is in fact superfluous to the function of the pot when used to handle milk. No other Pakot containers, to my knowledge, have a lip, which is why it is considered unusual. To generalize, the thing which is *pachigh*, in this case, is something pleasant to contemplate, strange or new and an embellishment. The pot is clearly not considered wholly beautiful and the utilitarian part is plainly distinguished conceptually from the pretty.

Subsequent investigations showed that, with the qualifications discussed further on, the following things were only *karam*, i.e., had useful functions that were non-aesthetic: clay cooking pots, shoes made from old rubber tires or cow hide, spears, headrests which are used as neck

pillows to protect the mens' clay headdresses, calabash containers, cotton sheets and other clothing, houses, water holes and cattle (except for one type). That cattle should be included in this list was surprising since they are the most highly valued of all goods and the attitude of the Pakot towards them might lead one to suppose that they would be considered beautiful.

The term *pachigh* (which refers to a state of being, a condition of a thing) can be applied to two classes of objects which, however, are not separated conceptually by Pakot. First are those things which are considered beautiful but are not made by the Pakot. These include the beauties of nature and objects of foreign manufacture, and in both cases what is beautiful is a part of something which is useful in some other way. For example, with one exception all cattle are *karam*, their value lying in the fact that they provide meat, milk, blood and certain by-products and that they are useful for obtaining other goods through trade and for 'buying' rights in other persons. The colours of the hides of these cattle are *pachigh*. A woman is also 'good,' but she may have aspects of beauty such as firm, round breasts, a light, chocolate-coloured skin, and white, even teeth. The glossy surface of 'american' cloth imported into the reserve is similarly considered pretty, but when it wears off the cloth becomes purely *karam*.

In regard to this last case, the common designation of art as man-made beauty,<sup>4</sup> the definition used here, in contrast to beauty occurring in nature, would exclude the glossy cloth as Pakot art since it is imported in that condition and not applied by Pakot. It may be art to the manufacturer but it is in the nature of a 'natural' occurrence to Pakot.

The second class of beautiful objects are those made or obtained by Pakot which are added to utilitarian objects by Pakot themselves. It includes paint which is made and applied to objects by Pakot and also coloured beads which are not made by Pakot but which are added by them to utilitarian goods for decoration. A special type of steer called a *kamar*, who is selected for certain admirable qualities and whose horns are warped by his owner, is considered to be wholly beautiful, unlike other cattle, and is kept somewhat like a pet and as a symbol of prestige to his owner. He is not put to subsistence use except under special circumstances and so is thought of by Pakot as an embellishment. Other objects in this class are cowry shells which are used to decorate various objects, polished wood surfaces as on spears or headrests, and bits of aluminium and iron or copper which are inlaid on the surface of the headrest to provide decoration. A design incised on any surface is also *pachigh*, as is a house if it is unusual in style or especially carefully and regularly built. Finally, a basket may have a pattern of weaving that is considered beautiful if it is unusual or if it comes from another district where the pattern of weaving is different from that of the area to which it is imported. Some of these objects are always separable from the things they enhance (e.g., cowry shells) and some are in a sense inseparable after they are added. But all are initially added to utilitarian things by Pakot and are not inherent in them. To reiterate, these *pachigh*

things seldom if ever exist of and by themselves but are used to decorate some utilitarian thing. This is not so clear in the case of the prize ox, but he may be regarded as being 'added' to his owner or as being an embellishment on his owner's herd.

We may summarize Pakot visual art as consisting of objects having purely aesthetic functions, including necklaces, headdresses and hairdress, pigments, polish or gloss, cowry shells, bits of polished iron and aluminium, iron and copper bracelets, ostrich feathers, the *kamar* steer, and unusual regularity and evenness in patterns or designs.

Informants sometimes refer without qualification to such things as a fully decorated adult man as 'beautiful,' but it is clear that they mean only the aesthetic embellishments. The Pakot tend to atomize the unit (or what might seem to be a unit to us) into its component pretty and non-pretty parts. Thus the term *pachigh* applies to the aesthetic components of a complex like the fully dressed adult man. In fact this atomization goes further, and the *pachigh* aspect may be broken down into its components. Thus a fully dressed man wears a headdress, necklaces, bracelets, etc. In contrast to the collection of aesthetic elements which are called collectively *pachigh*, any single element may be called *pachigha*, the final *a* in the morpheme being added as a modifier to show that the thing referred to is but one element in what may be thought of as a complex. Why this should be necessary is not understood. Neither is it known whether an element that may be *pachigha* in one context may be *pachigh* in another.

The Pakot concept of beauty is relative or a matter of degree. Any beautiful object may be viewed as more or less aesthetically pleasing than something else. Of three coloured shirts covered with designs which were shown to informants, the one with the brightest colours, the largest number of colours, and a wealth of surface pattern was considered prettier than the others. Of all cattle those coloured pure black are prettier than the others. This is true only for the locality in which the investigation took place where black cattle are relatively rare.

There is general agreement about the beauty of things in broad categories like colour. But while informants stated that all colours or pigments are pretty, the coloured hides of goats and sheep are not considered to be pretty in any way. Their colours are thought to be too drab and monotonous. Similarly, coloured beads arranged in a pattern are usually beautiful. But Pakot have preferences which exclude some arrangements. Some colours are preferred, such as blue in the locality under consideration, but any colour may be strung out in a solid line and be pretty. When different-coloured beads are strung on the same line an alternation of white and blue or of red and white is acceptable while alternation of red and yellow, red and blue, or yellow and white is not, apparently without regard to pattern. It would seem that the latter groupings are unacceptable because the contrast between colours is reduced and like the colours of goats and sheep they become monotonous. White and yellow provide little contrast but white and blue do.

Although there is general agreement on what is beautiful, there are areas of disagreement. We have already noted the regional variations in opinions about the relative beauty of cattle colours. There is sometimes disagreement about whether a thing is beautiful at all. A notable example is the case of a woman who felt that there was nothing beautiful about cattle, but that a healthy, green field of eleusine plants was beautiful. Most men would take just the opposite position. This difference of opinion apparently derives from the division of labour by sex. Women usually have little control over cattle, resent the menial labour associated with them which they must perform, and derive little prestige from them, while they can control the crops they produce and spend much of their time in the fields. In short, the men and women seem to find aspects of beauty in areas of life that interest them most and to which they willingly give attention. But at least one man was found who was a devoted cultivator and who described a field of eleusine plants as pretty because 'the plants are even and regular and green and when a man stands by the field he can look over all of them.' It was the panorama of all the plants which, unlike a field of straggling sorghum, can be easily viewed as a whole that appealed to him.

Allusion has been made a number of times to the beauty inherent in unique or unusual objects such as strangely woven baskets, unusually carefully built houses, or the lip on the milk pot. One informant said that European possessions were the prettiest things he had ever seen because he had never seen anything like them before. But not all things that are strange are necessarily beautiful. We have already seen that some strange arrangements of beads are not pretty. Some things which are unusual at first may acquire some utilitarian use and become common, thus losing the quality of *pachigh*. A concrete bridge built by Europeans in the reserve a few years ago apparently was at first considered to be wholly pretty. Now that the Pakot depend on it to cross the river its beauty has been reduced to certain embellishments such as the 'battlements' located along the sides.

New things which are startlingly beautiful are called *wechigha*, while those which are ugly and frightening are *wechipachigha*. It was difficult to find any example of the latter other than the hypothetical case of a man walking down the road carrying his head under his arm, but there was emphatic agreement that this was *wechipachigha*. Not all strange things are thought of as either pretty or ugly. There is disagreement about innovations and no generalization seems possible, except perhaps that when a new item has obvious utilitarian use it is excluded from the area of beauty.

Taken as a whole the Pakot attitude to new things is not so strange. Even among ourselves uniqueness is often a quality that has aesthetic virtue, and like Pakot we may consider some new thing pretty, such as a late-model automobile, until it becomes common and its other functions become dominant. The principal difference between Pakot and ourselves is that new things are rarer among them. They idolize the *status quo* and do not encourage change. When an innovation appears it may be especially striking.

Throughout this paper we have spoken only of what I have called visual art. There is a suggestion that the term *pachigh* may be applied to such things as dances and songs but the evidence is too scanty to discuss.

To conclude, Pakot visual art, defined as man-made embellishments with aesthetic appeal, consists essentially of the decoration of objects with no aesthetic qualities. Objects of art are things which are glossy or polished, have an unusual pattern or form (including strange baskets and finely built houses as well as the *kamar* steer) and colours. There are exceptions in that some unusual forms are ugly according to Pakot interpretations and drab colours are not pretty. Further, it seems to be generally true that any form which is useful in getting a living or has some non-aesthetic function is not beautiful. One essential characteristic of the Pakot concept of beauty is that it is an embellishment on the ordinary non-aesthetic things of life. These objects of art seldom if ever stand alone; they are applied to other objects as decoration.

We have analysed the Pakot concept of beauty and have isolated their art according to it, using the definition of art as man-made beauty. It remains to consider the possible errors introduced by attempts to deduce aesthetic values in another culture. I myself provide a useful case in point because before the Pakot ideas of beauty were discovered I unwittingly indulged in such deduction. To some extent the deductive approach was successful in that such things as necklaces and bracelets were classified as art objects in agreement with the Pakot. This was probably due to the fact that to a certain extent Pakot and European standards of beauty coincide or that some standards are universal. But a European has a tendency to generalize beauty to a whole object on which embellishment had been made, and thus to fail to recognize the fine distinction that Pakot make between an object and its embellishments. Furthermore, some things which the Pakot consider aesthetically pleasing embellishments were missed, while some were considered beautiful which Pakot would not. Deductively the lip of the milk pot along with the pot was considered non-aesthetic. This proved to be wrong, the lip being considered by the Pakot as a pretty embellishment. On the other hand, the headrest was deductively classified as an object of art because, although it has non-aesthetic functions, it is carried about by its owner like a decorative

cane and is polished and decorated. To the Pakot only the gloss and incised or inlaid design are beautiful. A headrest without these is not beautiful in any way.

This discussion would be incomplete if it were not said that although it may be useful for purposes of ethnography to isolate according to a universal definition the particular area of life of the Pakot that may be called 'art,' a classification of this kind is liable to be very misleading if not qualified by Pakot concepts of beauty. Pakot do not recognize anything called art as such. There is mere *pachigh* and non-*pachigh* whether man-made or occurring in nature. Our attempts to separate the two for purposes of this paper were highly artificial, in some cases dubious, and a violation of Pakot conceptualization of the universe. In short, we might argue that analysis of Pakot culture would proceed more adequately with a category of 'beautiful' or 'aesthetic' things than with a category of 'art.'

Our discussion suggests that attempts to classify the art of a non-literate people deductively without determining at first their concepts of beauty are bound to be only partly accurate. On the positive side, securing such information can directly contribute to art theory, as in this case to the old debate over whether the art of non-literate people is utilitarian or not. As we have seen, Pakot art is never utilitarian if we define utilitarian as having any non-aesthetic function. Beautiful things have only the function of pleasing the eye and only the function of enhancing non-aesthetic things.

#### Notes

<sup>1</sup> Franz Boas, *Primitive Art*, Dover Publications, 1955, p. 9.

<sup>2</sup> For example see Leonhard Adam, *Primitive Art*, London (Penguin Books), 1949 (revised edition), p. 25; or L. Beals and H. Hoijer, *An Introduction to Anthropology*, New York (Macmillan), 1953, p. 539. These examples could, of course, be multiplied greatly.

<sup>3</sup> The term *Pakot* is the plural form of which the singular is *Pachon*; to avoid unnecessary confusion only the plural is used in this paper. The Pakot, more commonly known as Suk, inhabit, in the main, the West Suk District of Kenya and belong to the pastoral Nilotc group of tribes of East Africa, being most nearly related to the Nandi. The research upon which this paper rests was carried out in the Orotu area of West Suk in 1951-52 under grants from the United States State Department (Fulbright Act), the Social Science Research Council and the Program of African Studies of Northwestern University.

<sup>4</sup> Boas, *op. cit.*, p. 12.

## ROYAL ANTHROPOLOGICAL INSTITUTE PROCEEDINGS

### The Work of the Ethnomusicology Committee of the Royal Anthropological Institute.

**IO9** The Ethnomusicology Committee of the Institute was appointed by the Council on 6 August, 1953, for the purpose of promoting the growth of this branch of anthropology in conjunction with interested bodies elsewhere, and especially of encouraging the use of ethnomusicological techniques by field anthropologists of all kinds.

The membership of the committee at the close of the session 1955-1956 is: the President, Joint Hon. Secretaries, Hon. Treasurer

and Hon. Editor of the Institute (*ex officio*), Dr. A. A. Baker, Mr. H. J. Brauholtz, C.B.E., Dr. P. J. Bohannan, Dr. G. H. S. Bushnell, Mr. R. E. Clausen, Mr. H. G. Farmer, Professor R. Firth, F.B.A., Professor M. Fortes, Professor C. von Fürer-Haimendorf, Mr. F. Howes, the Revd. A. M. Jones, Miss M. Karpeles, Dr. J. Layard, Mr. T. K. Penniman, Mr. D. Ryecroft, Dr. O. W. Samson, Mr. P. Saul, Professor A. N. Tucker and Miss B. de Zoete. The present chairman of the committee is Professor Firth and the Hon. Secretary Mr. Clausen. In addition to the full members, the following have been appointed Corre-

sponding Members: Mr. J. A. R. Blacking, Mr. J. F. Carrington, Dr. S. D. Cudjoe, Dr. E. Emsheimer, Professor M. J. Herskovits, Dr. H. Hickmann, Professor K. G. Izikowitz, Professor P. R. Kirby, Dr. J. Kunst, Dr. H. Pepper, Dr. C. Sachs, Dr. A. Schaeffner, Professor E. F. Seemann, Dr. Mme H. Subandrio, Mr. H. Tracey and Dr. K. P. Wachsmann. From its inception the committee has maintained close relations with the then recently formed British Institute of Recorded Sound (of which the R.A.I. was one of the founding societies, and which is represented on the committee by Mr. Howes and Mr. Saul).

At one of several meetings held this summer the committee considered the need for a central depository for ethnographical recordings in Great Britain, and resolved to recommend to the Council of the R.A.I. that the British Institute of Recorded Sound be recognized by the R.A.I. as the most suitable depository. The Council at its next meeting warmly approved this recommendation and was glad to learn that the British Institute was willing to set up such a depository.

The aim of the B.I.R.S., which has recently acquired premises of its own at the north-east corner of the British Museum site in Bloomsbury, is to form a collection of records which may eventually become as comprehensive as the British Museum Library of books.

The Council of the R.A.I. endorses the B.I.R.S.'s appeal to collectors to offer any discs, tapes or other records of ethnological interest for preservation in the Institute's archive; where collectors desire to keep their original records, it is hoped that they will allow the Institute to make copies. In approved cases the B.I.R.S. would—after any necessary permissions had been obtained—endeavour to make copies available to other institutions where desired. The B.I.R.S. will endeavour to maintain a register of work in progress which involves the use of recordings in the field or for subsequent research, and the Council of the R.A.I. appeals to all institutions and individuals engaged in work of this nature to co-operate by sending full information to the B.I.R.S. The B.I.R.S. is to circulate a bulletin to its subscribers in which information about records acquired and about work involving the use of records will be included. Any records or information should be sent to the Secretary, British Institute of Recorded Sound, 38, Russell Square, London, W.C.1 (telephone: MUSEum 4507).

Besides encouraging the proper use of recording techniques, the Ethnomusicology Committee wishes to draw special attention to the value of associating with recordings of music the actual instruments on which the music was made; the collecting of musical instruments is of course as old as ethnographical museums, but they have most usually been collected for their morphological

rather than their musical interest and are therefore seldom documented from the musical point of view. One of the purposes of the committee is to foster better collecting methods in the future and fuller use of existing collections for musicological purposes. The Department of Ethnography of the British Museum, which doubtless has the largest collection of tribal musical instruments in the British Commonwealth, as well as good facilities for the repair and maintenance of specimens of all kinds, is at present engaged on complete reorganization of its stored collections on a regional and tribal basis, with musical instruments (like other specimens) stored typologically within each tribal category; this will greatly facilitate reference to the collections by musicologists, and the contiguity of the B.I.R.S. premises on the same site will no doubt facilitate in the future some form of close association between the instruments and the recordings held in the depository. The ethnographical museums at Oxford and Cambridge (both represented on the committee) are taking increasing interest in the musical aspects of their instruments. Also, the Horniman Museum (London, S.E. 23) has become a centre for collecting European as well as ethnographical musical instruments. Its Curator informs the committee that, wherever possible, the instruments are kept in playing condition, with an ethnomusicologist in charge; concerts are arranged for the winter months and duplicate instruments are lent for public performances; the museum, he adds, would be glad to acquire any single instrument or collection of instruments, or tools used in their making, films, photographs and other documentation, and particularly welcomes such material from field ethnologists and collectors.

The committee is now engaged upon the collection and collation of the views of its members on the compilation of a brief compendium of advice for the non-specialist fieldworker who has opportunities of recording music and dances. Account will be taken of the *Manual for Folk Music Collectors* published by the International Folk Music Council, and of a valuable article on field recording in Africa published by Mr. Hugh Tracey in *African Music*, Vol. I, No. 2 (1955). When this task is complete, it is hoped that the result will be published, in MAN or elsewhere according to length. In the meantime, the Committee has some duplicated copies available of a draft memorandum of 'Simple Advice for Recording Music and Dances in the Field' compiled for it by the Revd. A. M. Jones on the basis of his South-East African experience, and these may be obtained by interested fieldworkers on application to the Hon. Secretary of the committee at the Institute, price one shilling plus postage.

The Hon. Secretary will be glad to hear from anyone interested in the committee's work.

## SHORTER NOTE

**Zande Totems.** By Professor E. E. Evans-Pritchard, Institute of Social Anthropology, University of Oxford

110 Though much has been written about totemism such theories as have been put forward regarding it have not paid sufficient attention to the forms which it takes on the African continent. I discuss here, however, only one particular respect in which such a study may be valuable. Most parts of Africa south of the Sahara display a great variety of animal and plant species, and since only some, usually only a very small proportion, of them are totems one may inquire whether any general conclusions can be reached about their selection and any principles be discovered in it. The purpose of this note is to set forth the full list of totems of the Azande of the Sudan and to make some observations about them. It is

essential that the list be as full as can be, because it is as important to know what are not totems as to know what are totems. The Azande are so numerous a people, are so ethnically mixed, and occupy so extensive a territory that it might be thought unwise to claim that all their totems are included in the list below, but the inquiry conducted by myself and my Zande clerk Reuben Rikita covered a large part, though by no means all, of Sudan Zandeland, and the clans of some thousands of men and their totems were taken down, so that, so far as this region is concerned, it is unlikely that many totems escaped the net. It may well be the case, however, that there are totemic creatures among the Azande of the Belgian Congo and French Equatorial Africa which do not appear in the list below. That even in the Sudan alone it is possible to compile so formidable a list of totems is

doubtless to be accounted for by the fact that many peoples of different ethnic origins have been assimilated, partially or entirely, in the Zande amalgam.

**Mammals.** (1) Lion (*bahu*). (2) Leopard (*moma*). (3) Hyena (*gangbu*). (4) Hyena (*zege*). (5) Genet (*mbili*). (6) Serval (*ngesfu*). (7) Civet cat (*tia*). (8) Cat (*paka*). (9) Cat (*derimvugo*). (10) Cat (*dagbura*). (11) Ape, probably chimpanzee (*gazuru*). (12) Ape, perhaps the same as no. 11 (*hawamu*). (13) Ape, perhaps the same as no. 11 (*gadayo*). (14) Ape, perhaps the same as no. 11 (*tangua*). (15) Dog-faced baboon (*waku*). (16) Colobus monkey (*mvugo*). (17) White-nosed monkey (*rimo*). (18) Monkey, small grey (*mbiro*). (19) Monkey (*ngarangara*). (20) Monkey (*bagianya*). (21) Monkey, grey (*balibo*). (22) Buffalo (*gbe*). (23) Giraffe (*dikara*). (24) Waterbuck (*mbaga*). (25) Bushbuck (*gbodi*). (26) Dig-dig (*ghafu*). (27) Mouse-coloured duiker (*mvuru*). (28) Gazelle (*ghangbalia*). (29) Bongo (*mangana*). (30) Reedbuck (*ngbandua*). (31) Roan antelope (*bisa*). (32) Rhinoceros (*kanga*). (33) Ant-bear (*garawa*). (34) Warthog (*zigha*). (35) Red pig (*zumburu*). (36) Red pig, probably the same as no. 35 (*zukubile*). (37) Domesticated dog (*ango*). (38) Jackal (*hua*). (39) Hunting dog (*wuru*). (40) Otter (*ngurunguru*). (41) Mole (*tiendua*). (42) Red field rat (*ndogo*). (43) Striped field rat (*sika*). (44) Field rat (*ghangata*). (45) Tree rat (*kuru*). (46) Cane rat (*lenvo*). (47) Rat, large-eyed (*tura*). (48) Rat (*bagbaya*). (49) Rat, evil-smelling (*ndari*). (50) Rat, short-tailed (*kandu*). (51) Ground squirrel (*badari*). (52) Ground squirrel, perhaps the same as no. 51 (*gambe*). (53) Hare (*ndakuta*). (54) Cricetus (*sumba*). (55) Bat (*fura*). (56) Water leopard (*moma imo*). (57) Thunder-beast (*gumba*).

**Birds.** (1) Vulture (*nguku*). (2) Vulture (*ranga*). (3) Vulture (*nguali*). (4) Vulture (*gbugbu*). (5) Greater bustard (*baziregbodi*). (6) Guineafowl (*nzengu*). (7) Domesticated fowl (*kondo*). (8) Black-crested hawk (*bigi*). (9) Black stork (*gonyo*). (10) Kite (*bakiki*). (11) Hornbill, black and white casqued (*ngongo*). (12) Hornbill (*ndasi*). (13) Blue-crested turaco or plantain-eater (*kurungu*). (14) White-crested turaco or plantain-eater (*kuwuwa*). (15) Honey guide (*turuga*). (16) Green pigeon (*baoto*). (17) Laughing dove (*mbipo*). (18) Pied wagtail (*ngbia* or *ngbiangbia*). (19) Cisticola warbler (*titi*). (20) Bronze mannikin (*nzoro*). (21) Dybowski's twinspot (*indi*). (22) Small black and white bird (*kpiaokpia*).

**Reptiles.** (1) Crocodile (*ngondi*). (2) Iguana (*kare*). (3) Tortoise (*basimaru*). (4) Tortoise (*dagado*). (5) Tortoise (*dakada*). (6) Gymnastic house lizard (*kokoso*). (7) Lizard, large (*tangandu*). (8) Lizard, small (*gara*). (9) Lizard (*kengua*). (10) Lizard, large (*bandugu*). (11) Spitting cobra (*basuru*). (12) Snake, loud hissing (*bagaweye*). (13) Snake, grey and black (*ngbukpu*). (14) Black tree snake (*bikingi*). (15) Adder (*kpokouo*). (16) Adder (*mbirimvuru*). (17) Snake, black (*wo apipe*). (18) Green grass snake (*ngama*). (19) Python (*ghara*). (20) Snake (*rungbu*). (21) Snake, reddish-brown (*wo kpaga*). (22) Snake (*gbingbiti*). (23) Snake (*rmgbura*). (24) Snake (*bilimi*). (25) Snake (*wo tura*). (26) Snake (*wo gumba*). (27) Water snake (*ngambue*). (28) Rainbow snake (*wangu*). (29) Slowworm (*magingi*). (30) Slowworm? (*kpurandu*). (31) Chameleon (*mvua*). (32) Toad (*dari*). (33) Toad (*nguro*). (34) Frog (*indiko*).

**Crustaceans.** (1) Crab (*ngere*).

**Insects.** (1) Millepede (*kingoro*). (2) Red pinching ant (*dindo*). (3) Black soldier ant (*pipe*). (4) Fly (*zi*). (5) Hornet (*bando*). (6) Firefly (*nzengue*). (7) Locust (*yere*). (8) Mosquito (*ngungu*). (9) Termite (*ge*). (10) Species of termite (*aswo*). (11) Species of termite (*angbaimo*). (12) Insect or grub associated with pumpkins (*ngorombe*, *gbira boko*). (13) Butterfly (*sfufurasu*).

As will be observed, it has not always been possible to give a more precise description of the creature to which a name refers than is contained in a statement of the family or species to which it belongs, and even for some of these imprecise indications I am indebted to dictionaries.<sup>1</sup> The compilers of these were often, however, in no better position to go beyond native statements than I was, the creatures not being easily seen or, if seen, identified; and such entries as 'species of small lizard' and 'une des variétés de serpents' do not take us very far. Moreover, it is not always certain whether two different names refer to different species or to different varieties of a species or are different names for the same

species or variety: for example, whether *gazuru*, *hawamu*, *gadayo* and *tangua* are four different words for chimpanzee or refer to four different kinds of ape or monkey, and likewise whether *dagado*, *dakada* and *basimaru* are names for different sorts of tortoise or different names for the same tortoise. This uncertainty arises particularly among the Azande on account of their being an ethnic amalgam in which not only the Zande language but other languages are current, either in an assimilated form or still, to varying degrees, spoken in addition to the Zande tongue. Thus I have been told that *gbingbiti* is the Miangba word for the slow-worm, *bilimi* another foreign word for it, while the true Zande word for it is *magingi*; but, if this is so, all three words are current and all three were given as names of totems. I decided, therefore, to list all the words given as totems, even if this meant in some cases that the same creature was listed more than once and that consequently there are more totems listed than there are in reality; and I have done so partly also for the reason that in a certain sense the name, rather than what it refers to, is the totem for the person who holds it to be his totem, for Azande pay little, if any, attention to the totem animal itself. In a number of cases a man who says that such-and-such a creature is his totem may, on account of the movement of people from one latitude to another, rarely, or even never, have seen it. He has only heard its name, and this is necessarily the case where the totem is an imaginary creature.<sup>2</sup>

Some of the creatures may be supposed not to exist, though the experiences they stand for are, or may be, actual. The *ngambue*, a crested water snake, is an unlikely creature. The *wangu*, the rainbow snake, does not exist as a creature, nor, if we judge by Zande accounts of it, does the *moma imo*, the water leopard. Nor does the *gumba*, the thunder-beast, Azande speaking of thunder as though it were some sort of animal. However, in spite of their speaking of these imaginary creatures as creatures, Azande do not conceive of them as they conceive of ordinary creatures which they often see or which they know that others have often seen.

I have to make it clear that this note describes only the natural distribution of totems and not their social distribution. In my lists of Zande clans and totems some totems occur on almost every page, for example, the leopard, the colobus monkey and the *ringhu* snake, whereas hundreds of entries may be run through without the appearance of others; and some only appear, in a list of some thousands of persons, two or three times. This means that whilst some totems are totems of large clans, or of several clans, and are represented also throughout Zandeland, others are totems of single clans which are also insignificant in numbers and restricted to small localities, or even of individuals or families of foreign origin living among the Azande (for example, the domesticated dog is probably a totem only of a few members of the Baka people, and the laughing dove of a few members of the Bongo people). We cannot therefore fully judge the significance of the natural distribution of the totems till the further and laborious task has been undertaken of determining the proportions of the total Azande questioned who regard each of the creatures as their totem and of mapping the territorial distribution of these persons. When presented, as here, simply as a list of totems, it cannot be said that any principle of selection is revealed unless it be the negative one that among the 127 totemic species listed there is not a single plant.

I have only to add that I have excluded from the list the elephant (*mbara*) as I can only find it recorded once in my notes and the entry is queried; that I have included the termite with reluctance because, though a few men gave it as their totem, it is probably not a totem in the same sense as the other creatures but belongs to a number of creatures whose names are avoided by certain clans on account of some misfortune connected with them; and that I have not been able to identify with any certainty a few entries in

Reuben's lists. These uncertain names—uncertain because I neglected to check them at the time—are *nduringbe*, *ngerepe*, *ngime* and *bagayuge* or *yuge* or *baynge*. *Ngime* means 'smoke' and *yuge* means 'wind,' but, because these things are so unlike other Zande totems and because they appear in the lists so seldom, I exclude them till they have been confirmed by further inquiry.<sup>3</sup>

## Notes

<sup>1</sup> C. R. Lagae and V. H. Vanden Plas, *La Langue des Azande*,

Vol. II, 1922; Canon and Mrs. E. C. Gore, *Zande and English Dictionary*, 1931. Some bird identifications were made from a typewritten list compiled by Dr. H. W. Woodman. Four creatures were identified by Mr. Raphael Zamoi, at present at Culham College. These are *halibo*, *ghughi*, *kpiakpia*, and *kpurundu*. For identification of the snake *bagawewe* I am indebted to my friend Major P. M. Larken.

<sup>2</sup> I have discussed this point in reference to Nuer totems in *Nuer Religion*, 1956, pp. 134f.

<sup>3</sup> Fr. F. Giorgetti tells me in a letter that *bagayuge* (*bayuge*, *yuge*) is another name for the snake listed as *bagawewe*.

## REVIEWS

## GENERAL

**The Tree of Culture.** By Ralph Linton. New York (Knopf), 1955. Pp. xvi, 692. Price \$5.75

**III** Ralph Linton was at work on this impressive volume at the time of his death and it was completed by his wife with the aid of his notes and transcripts of lectures. The title is rather startling for a modern work but Adelin Linton makes it clear in her admirable brief preface that the 'tree' intended is not of the old-fashioned spreading chestnut type but is the banyan tree in which the branches cross and fuse and send down roots which become in turn supporting trunks. It is this non-unilineal concept of cultural growth which pervades the volume.

Geoffrey Barraclough has recently, in *History in a Changing World*, Blackwell, 1956, recommended the comparative study of civilizations to historians, but even the wide scope he envisages would probably stop short of the canvas employed by Linton. After a short note on the physical development of *Homo sapiens*, he discusses several evolutionary processes and basic inventions, and then moves to a world survey of archaeological, ethnological and historical data. The scene shifts from Europe and Africa to the Orient and native America, and ranges equally widely in time. The treatment is fullest on just those areas and periods which the historian tends to neglect. It omits only the development of the Euro-Western nations—a subject with which history has dealt most fully and from which most of its theoretical orientation has derived. Eventually, historical theory may have to take cognizance of anthropological materials. In the meantime, such books as this and Carleton Coon's equally wide world survey (C. S. Coon, *The Story of Man*, New York (Knopf), 1954) serve to call attention to the possibilities. A play-by-play comparison of Linton and Coon would be of value since both are based on regular series of university lectures in anthropology. But it is well to remember that the particular conclusions reached by both men are tentative and subject to considerable correction. A review of their treatment of detail, or even of their reconstruction of cultural relations, would reveal numerous points of difference, and both the detail and the cultural connexions cited could be adversely criticized *ad nauseam*. What seems more important is to focus attention on the viewpoint implicit in anthropological interests of this kind.

The assumption seems to be twofold: (1) that the activities of man, as examined by the various branches of anthropology, are necessarily related, and (2) that the same essential processes are involved in the development of all men of all eras. The hypothesis which has not been validated in other materials and among other peoples is thus automatically suspect, and the two premises lead towards an analysis of the totality in which the unique is embedded. All of this is familiar enough in anthropology. It needs to be referred to here only because the view, used as it is in a survey of the historical development of mankind, tends to take little account of the traditionally 'historical.'

The differences lie not so much in the sources employed or the techniques utilized—for these are constantly becoming less differentiated—but in the assumptions. The historian not only studies events, or series of events, but he assumes that these events may be studied in isolation and he further assumes the validity of his interest in their idiosyncratic elements.

If our analysis has been correct, Barraclough is recommending the abandonment of studies in isolation. But the adoption of such a policy would not lead the historian to duplicate the anthropologist's effort. He would, in addition, have to recognize the necessary inter-relations of human phenomena, as does the anthropologist. And the anthropologist, in his turn, would have to admit the validity of the study of the idiosyncratic. Only by some such amalgam of assumptions may we hope to have any final picture of the development of civilization.

This review may seem to have gone unnecessarily far afield. The digression becomes meaningful if *The Tree of Culture* is to be viewed as more than an inadequate attempt to write history. Linton's great gift was in synthesis and he had himself worked in the field of cultural differences. One might expect from him, therefore, an attempt to create just that sort of amalgam of assumptions cited above. We have been deprived of this attempt, however, for the only conclusion consists of the transcript of a lecture delivered in 1948. Linton looked forward to this book as the climax to his life's work and all the vigour and the insight which made him such a great lecturer are here. But the final intellectual effort which might have lifted the work into the theoretical arena is lacking. Despite the excellent efforts which made the appearance of the book possible, this is not the full contribution he had once planned. Sickness and death have again cheated us.

MARIAN W. SMITH

**Cultural Anthropology: An Abridged Revision of Man and His Works.** By Melville J. Herskovits. New York (Knopf), 1955. Pp. xvi, 569, xxxiv, 15 plates, 59 text figs. Price \$5

**II2** In the Preface to this book Professor Herskovits says, This abridgement and revision of *Man and His Works* has been undertaken at the request of many of my fellow anthropologists, who, in making the point of view and the materials of the larger book available to their students, have felt that a briefer presentation would be more in accord with their needs in shorter elementary courses.

By skilful cutting of superfluous phrases and repetitious argument, and by extensive rearrangement and telescoping of chapters, the author has succeeded in producing a revised version which not only contains all the main features of the original volume, but also introduces new material on race and on the relation between physical type and learned behaviour. Despite these additions and subtractions it is essentially the same book as that reviewed in *MAN* by Dr. H. S. Harrison (1949, 4). Professor Herskovits' theoretical approach has not changed over the intervening years, and he does not make this an opportunity to discuss the criticisms which have been levelled against it. (Notably by Bidney in his book *Theoretical Anthropology*.)

In either its old or its new form this remains one of the best American textbooks in anthropology, and it provides a useful introduction to the many fields on which first year teaching is based. The appearance of this revised edition serves as a timely reminder that British anthropologists have not yet made good their promise to produce a textbook of social anthropology organized around theoretical principles more generally accepted in this country.

R. T. SMITH

**Research Films in Biology, Anthropology, Psychology and Medicine.** By A. R. Michaelis. New York (Academic Press), 1955. Pp. xvi, 490. Price \$10.

**II13** This book is a comprehensive survey of the achievements and possibilities of production, analysis and use of films in certain branches of research. There are nearly 400 pages of text, 88 figures and nearly 1,500 book, article and film references. Only the 70 pages applicable or devoted to anthropology are reviewed here.

In the chapter 'Human Record Films,' Dr. Michaelis takes up a commendably cautious position. He considers briefly some of the mechanical limitations of the cine-camera in observation, and weighs these against the different limitations of the human eye in the same situation. He discusses the conventions of the film, to which the fieldworker must inevitably be a party, and their effects on the final objectivity of the film record. He touches—all too briefly for the anthropologist interested in the possibilities of the cine-camera in fieldwork—on the use of the trained cinematographer. A little naïvely perhaps for many who have done anthropological fieldwork, but have little or no knowledge of cinematography, he concludes that 'the ideal solution is a research worker, equally competent in the techniques of both the subject and cinematography, though this is not always possible.' However, to confirm his optimism, Dr. Michaelis supplies a tyro's reading list in cinematography, suggests a number of elementary cinematographic exercises, and discusses some of the practical problems of film work, including sound registration, in the field.

In a subsequent chapter, Dr. Michaelis usefully lists, by regions, films of anthropological interest as well as agencies interested in films with anthropological content. He may be pardoned some errors of geography and ethnography, such as 'the Dongons in the Cameroons in French Equatorial Africa,' but would welcome his attention drawn to slips in his references: surely 'Riaule' should read 'Griaule'; 'Maprusi,' 'Mamprusi'? He often keeps, too, to foreign forms of tribal names, such as 'Massai.'

In general, Dr. Michaelis deplores the lack of attention given to cinematography by anthropologists at the present time. He finds that there is no systematic archival interest in anthropological films; little collaboration between fieldworkers and cinematographers, and only rudimentary consideration of cinematography as an adjunct of other field techniques. He might have added to his criticisms the haphazard way in which films are used in teaching the subject.

However, Dr. Michaelis' main concern is with cinematography as a research technique. He is here perhaps a little purist, a little biased by his own interests in cinematography as a natural scientist, for he sees the contribution of cinematography to anthropology largely in experimental terms. He might have made his criticisms more telling had he elaborated the part played by visual evidence in anthropological field research of all kinds, and considered the virtues and defects of the cine-camera as an extension of the human eye not only of the fieldworker, his colleagues and laymen, but of the subjects of the 'human document' film itself.

Dr. Michaelis' chapters on anthropology in *Research Films* will stimulate those who already believe that cinematography has something to contribute to anthropological research. His advice will encourage those who themselves wish to use this technique. As for those who readily advance adverse arguments of principle, or sug-

gest financial or practical difficulties—enthusiast though Dr. Michaelis is, he meets them half way. DERRICK J. STENNING

**Primates: Vol. II. Haplorhini: Tarsioidae.** By W. C. Osman Hill. Edinburgh (U.P.), 1955. Pp. xx, 347, 14 plates, 49 text figs. Price £3 3s.

**II14** This second volume of Dr. Hill's *Summa Primatologia* consists of two distinct though correlated parts. About the first third of the text, including the separately listed proper references to it, is given to a definition and general account of the Haplorhines. It might be described as an overture to the *Opera futura* of the author, containing sections which cover all the salient features of the topography, reproductive physiology and development of this grade of the primates. These sections furnish an excellent introduction to the subject of anthropoid morphology; they give what many of us had hoped to find in Sonnag's little book when we were learning the rudiments of this part of science. The part upon the Haplorhines generally concludes with a shrewd critique of what has been done and written in the sphere of their mental processes, followed by a brief statement of their taxonomy and temporal distribution.

The rest of the book deals wholly with one of the numerous pretenders to the throne of our ancestry—the Indonesian Tarsier and its extinct relatives. A comprehensive natural history and anatomy of the little beast occupies some two-thirds of this part; it is preceded by a formal definition and classification of the tarsioid sub-order and includes a very complete history of the discovery and recognition of *Tarsius* and its various species. The treatment follows the pattern indicated by the introductory part of the work. The accounts of tarsioid anatomy are based upon the author's own observations as well as those of others. They are very full indeed, and one human anatomist remote from the animal has no difficulty in building up a mental picture from them that is as good as he can ever hope to get from print and picture alone. The student of the history of zoological illustration will find an interesting series here, a succession reminiscent of the bestiary, of Albinus and of the sentimental school in turn (fig. 7, Plates V and VI). He can compare them with a modern photograph and with a skiagram of the complete animal.

Since *Tarsius* is now restricted to one genus and one world-region, all else in the book deals with the fossil families, real or reputed. The keys to their division into sub-families and into genera are all dental, as are indeed many of the tarsioid fragments that remain. The reader is left in no doubt about which finds are certainly assignable to a sub-family and which not. The European *incertae sedis* are listed at the end.

The book is worthy of its printers. Its value is enhanced by the illustrations which are mostly from the hand of Yvonne Hill; they are clear and good to look at, even closely. There is an adequate index of 16 close-printed columns and a two-page map of Tarsier's present realm. Saving book titles, the references to the literature give no indication of the scope of the papers referred to. This may be because 20 pages of references would otherwise have been expanded to 40, no small matter in these days of printer's costs. It is a small fault to find with a book which is classical in both endeavour and achievement, and is, moreover, an easy thing to read.

M. A. MACCONAILL

## AFRICA

**Trade and Politics in the Niger Delta, 1830-1885.** By K. O. Dike. Oxford (Clarendon Press) (London: Cumberlege), 1956. Pp. vii, 250. Price £1 10s.

**II15** Dr. Dike's long-awaited book marks the emancipation of Nigerian historiography from its 'feathered savages and dashing merchant adventurers' phase; and a most impressive beginning it is to serious studies in this field.

The book deals with the crucial period covering the beginnings of British political control in Nigeria. In the first three chapters we are given a sketch of the origins and social organization of the Delta states, principal African actors in Dr. Dike's drama, followed by a summary of their early trade relations with Europe; the rest of the

text is devoted to a detailed study of the economic and political events leading up from 1830 to the decision to claim and implement a formal Protectorate.

The author's principal thesis is that imperial expansion on the West Coast was never the primary aim of British policy-makers of the nineteenth century. These, by and large, were interested in the purely commercial possibilities of the Niger Basin; but they discovered that efficient commerce with politically unstable Delta partners could only be maintained by intervening in local politics to force their stability, and as a result we find them gradually drawn from *laissez-faire* into active colonialism. The inevitability of a like train of events wherever a powerful group engages in commerce

with partners at once less stable and less powerful is well shown here; for in the second chapter we see first how in the slaving days when the Aro Chuku people were vital links in the trade chain, they were forced to intervene in the politics of the Ibo hinterland to maintain such stability as was essential for the continuance of commerce, and again how, in the oil-trading days that followed, similar intervention was forced on Delta monarchs to ensure a regular yield from the oil markets. Commerce followed of necessity by intervention is clearly an old story on the West Coast, and one that gives rise to such detailed parallels as between the 'Market Governments' composed as to 50 per cent. of Ibo traders, 50 per cent. of Bonny men, and held in the shadow of a Bonny war canoe, and the 'Courts of Equity' composed as to 50 per cent. of Bonnys, 50 per cent. of Europeans, and held in the shadow of a British gunboat.

A second theme of the book is the peculiar nature of the contact between European and African cultures in this area. Although he hints that the 'House System' organization of the Delta states was in origin a direct outcome of the pattern of human relationships required by the coastal trade, the author points out how little indigenous social values changed through 400 years of subsequent commerce. This basic lack of Europeanization is something that surprises even contemporary visitors to this part of Nigeria longest in contact with Europe; and Dr. Dike rightly, I think, attributes it largely to the narrowness of aims and outlook of the 'rough diamond' merchants who despite themselves were the sole ambassadors of European culture through the peak years of Delta prosperity, and who were only succeeded by men concerned with enlightening as well as purse-lightening when the twin discoveries of steam power and quinine made practicable the opening-up of the interior at the turn of the nineteenth century.

Whilst acknowledging the validity and importance of these two main points, one can make several criticisms on subsidiary matters. First, a great deal of space is devoted to Bonny and Calabar at the expense of such peoples of the western Delta as the Western Ijo and Jekri (Itsekiri)—of whom the latter at least traded actively with Europe throughout this period. True, Bonny and Calabar were in the main stream of events leading up to the proclamation of the Protectorate, whilst the western Delta was something of a back-water in this respect; but comparisons of the political organizations and responses to commerce of all the various peoples of the Delta would nevertheless have been illuminating. Thus, detailed structural parallels between the 'House System' organization of the trading states and the 'democratic' organization of the Western Ijo, together with cultural affinities and traditions of origin linking the former to the latter, provide evidence for Dr. Dike's otherwise barely supported view that the 'House System' evolved from the 'democratic' type of organization in response to the advent of commerce and economic differentiation; and Western Ijo records add to this by providing a striking though temporary example of such a social revolution within the period dealt with by the book—in the case of Ekede of the Mein clan, who by brilliant use of trading opportunities about the turn of the nineteenth century, became virtual dictator of an erstwhile democratic community which he transformed for the space of a few years into a replica of one of the eastern 'House Systems.'

Secondly, great emphasis is placed on the 'divinity' of the King of Bonny in analysing the causes of the latter's political disintegration. Thus we are told that the exile of King William Dappa by the British antagonized even the anti-monarchist faction in Bonny, because it was a flouting of supernatural authority. Yet there is a Bonny tradition that one Owsa had previously been deposed in favour of the great Pelekule because he was too poor to support the state as a king was expected to, so that deposition had a clear customary precedent. One would have liked to know in much more detail how far the office and decisions of the king were backed by supernatural sanctions—especially since some sources (e.g. the writer of the Bonny Intelligence Report) seem doubtful whether they were so backed at all.

These points apart, Dr. Dike's is a masterly book which shows that rare historian's virtue of combining sustained liveliness with a non-partisan approach. As such, it will prove as absorbing to lay 'coasters' as to historians and anthropologists—though an extra com-

mendation is due to the specialist for the comprehensive listing of sources and for the watchful use of indigenous traditions whose sociological function is often anything but that of objective recording.

W. R. G. HORTON

**The Study of African Art.** By William Fagg. With illustrated catalogue of an exhibition of African art. Allen Memorial Art Mus. Bull., Vol. XIII, No. 2. Oberlin, Ohio (Oberlin College), 1956. Pp. 43–63, 92 plates. Price \$1. **African Tribal Sculpture.** By Margaret Plass. Philadelphia (Univ. Mus.), 1956. Pp. 57, 62 plates, map. Price \$2

Mr. Fagg's Oberlin College lecture is an important contribution in that it challenges us to consider carefully the various approaches to the study of African art.

It is possible that a special study of any art period or school, whether it be Byzantine or Flemish, Celtic or Japanese, would really present the same problems if we were sensitive enough to notice them, but we have ceased to realize the need for fully understanding Greek mythology in order to appreciate Greek sculpture, or for delving deeply into early Christian symbolism before we sit down to enjoy half an hour with the Book of Kells. The study of African art, on the other hand, is still young enough for us to feel self-conscious and insecure about it. It is well that this is so, for our knowledge of the subject can at present only be partial, and it would be fatal for any of us to attempt to dogmatize without due humility.

As Mr. Fagg points out, African art has been taken up enthusiastically first by the ethnologist, then by the artist, and finally by the poseur who indulges in what he delightfully calls *négrerie*. This last can be written off as a negligible influence, for fashion has always had its passing crazes, and does little to harm or enhance solid worth. The early collectors, some going far back to the seventeenth century, were often men of taste who brought home works of art of great interest and value, although they talked neither of the magico-religious significance nor of the formal values of the pieces which they collected. Then for many years the ethnologist held the field, and did sterling work in correlating the masks and sculptures with the background pattern of life from which they came. For in so far as a great part of African sculpture holds for its maker or owner a religious-utilitarian value rather than an aesthetic one it is necessary that we should have some understanding of its background if we are to be in any way emotionally attuned to its aims and its attainment. The ethnologist's approach was essentially deductive and scientific; he was not, as an ethnologist, moved by beauty of form.

When the Post-Impressionists in their turn 'discovered' African art in the light of their own inductive gifts, they rather naturally closed their ears to all this scientific analysis in which the ethnologist delighted. The value to them of a work of art lay in the emotional impact which its form or pattern or texture made upon an observer who was trained in aesthetic appreciation. Both of these approaches are valid, but they must be realized as being complementary rather than conflicting ways of study, and it is this synthesis which is so difficult to attain. Only too often the writer on African art, whether he approaches his subject from the scientific or the aesthetic point of view, holds his little magnifying glass over his favourite aspect, enlarging it out of all proportion to the rest, so that we can get no all-round conception of the worth of a piece of sculpture.

Mr. Fagg discusses some of the branches of study necessary in a holistic discipline of African or any other tribal art. It is here that I have my only quarrel with him, for he entirely omits the approach which must come first to the artist; the studied appreciation of form, colour, pattern, texture and other aesthetic values. For us these must come first, although we fully admit that we must then enrich our appreciation by critical scientific study of all that lies behind the work. I have spoken of studied appreciation, and I mean studied, something far more serious than a rapid emotional reaction to a work, a definite analysis of its aesthetic qualities.

Mr. Fagg lists among his subjects for study the place of African art in world art history. Here he divides art into two groups which he calls those of industrial and pre-industrial cultures, taking as his criterion of an industrial culture one which is concerned with buildings or other constructions which by their nature call for precision in design or execution. This is a useful and workable division, but I

am not sure whether the title alone conveys his meaning. He shows how difficult it is to work through the normal historical methods of study, and emphasizes the need both for the study of living carvers at work, and for the collection of well documented photographs of sculpture. He then stresses the importance of the study of the ecological background, of economics and social structure, and religious and philosophical concepts.

From these primary studies, which must consist chiefly in the collection of material and relevant data, the writer leads us on to the consideration of their interpretation. He describes the methods of stylistic analysis carried out by the Belgian school under Professor Olbrechts and suggests that the scope of such an approach should cover not only the identification of tribal sculpture but that of the work of individual artists, and the history of development of various tribal 'schools.'

Such specialist study will best be carried out in the larger museums where adequate collections of work from various African territories are housed; but both the specialist worker and the student interested in African art as a whole will continually need the stimulus of seeing work which has been brought together from over the whole continent, or those parts of it where carving of a sufficient standard is to be found. The exhibition of African Tribal Sculpture shown in the University Museum, Philadelphia, from April to September, 1956, should certainly make a rich contribution towards this end. The work exhibited has been brought together from museums and private collections in Europe, Africa and America by Mrs. Webster Plass, some 40 of the pieces coming from the Plass collection which is on loan in the British Museum.

It is difficult to make any assessment of an exhibition which one has not seen, but from the catalogue and from all information available in this country the exhibition would appear to be one of the most important since that held in the Museum of Modern Art, New York, in 1935. The pieces have been chosen not only to give a résumé of the characteristics of the principal 'schools' of African tribal art, but more especially to show only works of a high aesthetic standard. That such a standard has been achieved is clear from reproductions in the catalogue; and it would be of great interest to compare the works shown from the private collections with those which have become familiar to us in the public museums and which we have now come to regard as the classics of African art.

The catalogue has been well got up in the format to which we have become accustomed in exhibitions of art of the British Colonies held in this country; although the accidental omission of photographs of two or three starred pieces is to be regretted. Each section is headed by a brief description of the tribe concerned followed by what documentation is available of the relevant work shown. These are clearly written without too many technical terms. Judgments of aesthetic value are usually omitted, and rightly so, for in this matter the pieces must speak for themselves; they cannot be assessed in the small space available.

The whole question of the format of an exhibition catalogue is a difficult one. Such books when well illustrated and documented hold an important place in the library of the serious student of African art, and for him the brief references to style, technique, or social background will be intelligible and valuable. But they also

have to appeal to the general public, who may not have the necessary background knowledge to follow such references. For them the format used in the catalogue written by J. J. Sweeney of the 1935 exhibition in the Museum of Modern Art, or 'The Arts in the Belgian Congo and Ruanda-Urundi,' published by the Information and Documentation Centre of that country, or the catalogue of the Allen Memorial, Art Museum Exhibition discussed above, each with its essay or collection of essays on some aspect of study of African art, would be of very great value, as they would to us all.

K. M. TROWELL

**Au Sahara: Les Hommes et leurs Outils, Vol. I.** By Jean Gabus. Neuchâtel (*La Baconnière*), 1955. Pp. 106. Price 24 Swiss francs

Between 1942 and 1953, staff of the Musée d'Ethnographie de Neuchâtel under Jean Gabus made eight expeditions of three to six months each to various parts of the Sahara and Western Sudan for the collection of representative samples of the material culture of the peoples of the area—Moors, Tuareg, Fulani and Hausa. The results are there for all to see in the excellent and long established African collections at Neuchâtel. Photographic, film and sound documentation has also been made, and some of this has been seen and heard in Great Britain. An interesting feature of the expeditions is that they were made by aircraft, and a discussion of the utility of this form of transport for work of this kind has also been published.

This volume is the first of a series illustrating the work of the expeditions; Volume II is to be entitled *Les Arts et les Bijoux*, Volume III, *Les Techniques et le Métier*. Volume I, at any rate, is in the best sense a souvenir volume. It is beautifully reproduced, with excellent photographs by the author, clear maps by André Chautems, and a delightful cover designed by Hans Erni. Its style is impressionistic, its tone evocative. We are transported across the Sahara with its varied faces, and introduced to the freemasonry of the South Saharan nomad. Attention is focused on the trading places on the southern shore of the sand sea—the markets of the Western Sudan. Tahoua is chosen as an example, and with the aid of excellent diagrams a general description is given of market activities, depending basically upon the exchange of desert salt for savanna grain. Gabus comments regretfully on the fate of the faded cities of the Western Sudan—Timbuktu, the erstwhile centre of Islamic learning and piety, and Walata, the once famous commercial centre, now 'dying with dignity.' The aesthetic as well as practical contribution to the cultural life of the area made by the crafts is described. There follows a brief description of the entertainment world of the Western Sudanese markets—the dancing girls, prostitutes, praise singers, jesters and musicians. Finally there are ethnographical vignettes of the Tuareg, the Pastoral Fulani and the Nemadi. These are interesting, not for depth of perception, or ethnographical precision, but for a ready and engaging sympathy for the way of life of Saharan and Sudanese nomads. This nostalgic atmosphere runs frankly through the whole book, text and illustration alike. Those who already know something of the Sahara and Western Sudan, or those who can merely savour nostalgia, will enjoy reading or looking at the pages of *Au Sahara*.

DERRICK J. STENNING

## AMERICA

**The Little Community: Viewpoints for the Study of a Human Whole.** By Robert Redfield. Chicago (U.P.) (U.K. agents: C.U.P.), 1955. Pp. 182. Price £1 10s.

II8 The study of small-scale societies constitutes one of the dominant traditions in anthropology. It also accounts for a formidable library of monographs that neatly pigeonhole a vast array of societies which seemingly have no relationship to each other. The recent concern for comparative studies indicates an interest in carrying the accumulated legacy beyond the discrete social unit. Catalogues of 'cultural universals' are one attempt in this direction. Another direction is the comparative study of societies as whole units that can only be meaningfully understood in relation to each other so long as a sense of society is retained.

In *The Little Community* Redfield addresses himself to the issue

of comprehending societies as whole units. It is, however, unfortunate for a comparative anthropology that, while emphasizing that little communities are and have been whole units and can be studied as whole units, Redfield does not deal with society. This, at least on the face of it, appears to be the case since he explicitly notifies the reader that his is solely a study of the methods used to study little societies holistically. However, in carrying out his task, he introduces a number of illustrative societies (Nuer, Tale, Siriono, Skolt Lapp, Plainville, Chan Kom, Tepoztlán and others) which he analyses substantively and comparatively in rich detail along a number of dimensions—ecology, social structure, group personality, world view, historical theme. But since the problem is always how adequately each dimension portrays the whole, the case by case substantive comparisons do not materialize into any observations

on the general character of small scale societies. By a continuous shifting back and forth between substance and method for the purpose of establishing the relative usefulness of various methods, the book is not a contribution to comparative anthropology. Nor is it a contribution to comparative method since the illustrative material was selected because of its substantive interest rather than on impartial 'scientific' grounds.

Redfield concludes that anthropology does offer a satisfactory method for studies of whole communities; the necessary and essential ingredients for the study of whole societies is provided by the method of the rural-urban model.

By definition, rural or tribal society is whole when there is uniformity of conscience and world view, stability of career lines over generations, equivalence between aspiration and achievement and close correspondence between ideal and action. By this definition it is apparent that there is a close correspondence between the integrated society and the possibility of applying holistic concepts. But, in building the rural pole on a conception of the stable society, using Maine as a starting point and guide line, one may work in a way consistent with an intellectual tradition, but inconsistent with contemporary facts.

The issue is made clear by Lewis's re-study of Tepoztlán and the reply which Redfield makes to it in this book. The two studies

on the same community describe two wholly different set-ups. In what is really a methodologically irrefutable position, Redfield resolves the contradiction between his and Lewis's observations on Tepoztlán by maintaining that a single whole can be viewed either wholly or in parts from a variety of positions, these being a function of curiosity and models. By this argument, anthropology very nearly becomes a reflex of itself, and the study of community wholes a matter of aesthetics. Typologies if they are to mean anything must necessarily be linked to a concrete empirical reality.

The studies of societies as wholes can and has led to interesting formulations and studies, a fact which is amply illustrated by this book. However, by an initial focus on the whole, one's attention is necessarily directed away from the parts which in some cases may be in conflict or tension with the whole or autonomous in relation to it. In looking at the totality, there is a tendency to assume a general social integration whose appearance may be given by a single institution; where kinship dominates, unity may be given by the authority of the kinship head; the measure of Zuni integration seems clearly to be related to the role of the priests. While it is true that integration is a legitimate subject of study, it is also true that one can confuse it even in small societies with what is a rational calculation of interest.

ARTHUR J. VIDICH

## ASIA

**Village India: Studies in the Little Community.** Edited by McKim Marriott. Chicago (U.P.) (U.K. agents: C.U.P.), 1955. Pp. xx, 269. Price £1 1s.

The study of present-day Indian society and culture by social anthropologists has been slow in starting, but, following the planned area studies of certain American universities, co-operating with Indian universities and supported by the big American Foundations, a significant beginning has been made. The significance is illustrated in the two chief emphases in these studies. One is the nature and process and pace of social change in Indian villages, changes stimulated by political, economic and social movements of the last 10-20 years. The other is the relation of the small scale culture of an Indian village to the broad sweep of Hindu culture throughout India.

The eight authors have all done recent fieldwork in Indian villages reaching from the Tanjore district of Madras in the south to the Delhi district in the north. With one exception, the papers were discussed at a seminar in the University of Chicago, conducted by Professor Robert Redfield and Milton Singer, and were subsequently revised in the light of comments and discussion. In this sense there has been collaboration among the authors, and also in another sense, since Professor Redfield circulated among the authors an outline of his lectures at Uppsala University on *The Little Community*.

An assumption running through these papers is the prevalence of a body of beliefs and ritual practices and of social patterns called Indian civilization. Each author deals in his own way with the variants of this prevailing Hindu culture as seen in his village, and links 'this great tradition,' as one of them calls it, to 'the little tradition' which is peculiar to the local culture. Four of the papers are concerned mainly with social structure, in its traditional and in its changing form. Two of the papers approach the main problems through the study of ritual, and one through the study of personality formation in a Gujarat village with strong Rajput traditions. In another paper, by comparing the society in a Mexican village with that of a Punjab village, the author illustrates the wide range of variation between peasant societies in different cultures.

MARGARET READ

**Village Life in Modern Thailand.** By John E. de Young. Berkeley, Los Angeles (U. of Calif. P.) (U.K. agents: C.U.P.), 1955. Pp. 225. Price £1 6s.

Professor de Young, who has done a year's detailed fieldwork in a village of North Thailand, and has spent a further two years in other parts of the country, has decided to write generally of Thailand peasants, rather than to analyse one village, or the villages of a single area, in the style of social anthropology. This

attractively-produced book gives an unpretentious and clearly-written list of the various aspects of Thailand rural 'culture,' arranged in six chapters: Village Organization—in fact mainly about Thailand; Social Organization—about such matters as houses, households, hygiene and so on; the Life History of the Individual—birth, school, conscription, kinship and marriage, old age; Agricultural and Economic Patterns; Religious Beliefs and Practices; and, lastly, the Changing Scope of the Villager's World, in which we learn something of development and development programmes. It seems clear to one who knows little of Thailand that the facts have been carefully checked and sources treated with due scepticism, but inevitably such a book is full of general statements about all Thais, and in spite of the author's evident caution, one wonders how widely some of these hold. Moreover, Professor de Young can only cover so wide a field by treating the parts briefly and at times superficially. For example, the predominantly matrilocal residence pattern suggests all sorts of interesting kinship problems, which Professor de Young does not even mention; and the relations between each village and its Buddhist monastery are described generally as a fact of 'Thai culture,' which Professor de Young does not seem to think calls for more analysis or explanation. Perhaps we may hope for publication of the detailed results of his northern study in a more sociological vein. The present work is certainly extremely informative, and has a useful index and bibliography, and some demographic appendices.

PAUL STIRLING

**Studies in Cheremis: The Supernatural.** By Thomas A. Sebeok and Frances J. Ingemann. Viking Fund Publ. in Anthrop., No. XXII. New York, 1956. Pp. 357. Price \$5

The preface of the work shows that many persons other than those mentioned on the cover have participated in its compilation. The authors have compiled all notes concerning the beliefs and religious thinking of the Cheremis which they have found in earlier published Cheremis material. There are, however, several works containing important information about the Cheremis which are lacking among the sources quoted in the bibliography (e.g. A. Olearius, P. J. v. Strahlenberg, P. S. Pallas, J. G. Georgi, N. Rychkov, G. Gorodskoy, A. F. Rittikh, N. Troitskaya, V. Mendarov, P. E. Yerusalnov, N. V. Nikolskiy, A. Apriyev), while several of those cited (e.g. Carnap, Toynbee, etc.) could well be omitted. As for the anthropology of the Cheremis (p. 20), the description by Kajava in *Suomen Suku*, Vol. II, pp. 292ff., is fundamental.

The system of quoting the sources is also unsatisfactory in other respects. E.g., the chapter on the historical background is obviously for a great part (pp. 24-28) a verbatim translation of the outline

history of the Cheremis by Wichmann in the Finnish encyclopædia *Iso Tietosanakirja* (1917), Vol. IX, col. 1866ff. (reprinted in *Suomen Suku*); this source, however, is mentioned neither in the text nor in the bibliography. The above encyclopædia would have supplied photographs of Cheremis housing far superior to the drawings by Heikel reproduced on pp. 36f. Besides, the description of Cheremis paganism by Holmberg-Harva contains a number of most valuable photographs of ceremonies and rites, etc., which would have been instructive in this work also.

The religious concepts of the Cheremis are of special interest, since paganism there has survived until our times (the official Russian census of 1897 designated over 27 per cent. of the Cheremis as pagans, and also many orthodox were only nominally Christians), while e.g. their near relatives, the Mordvins, were all at least nominally Christians before the end of the eighteenth century. Though numerous criticisms on various details of the work of Sebeok and Ingemann must be made, its main part, 'Encyclopædia of the Sacred' (pp. 45-257), is a very valuable and easily accessible collection of materials for all further studies of this religion. All Cheremis notions related to the supernatural are entered alphabetically, classified according to their subjects. The 'supernatural' character of certain items, however, is very slight, e.g. p. 207 G (= 'Omens'), 13. 4. 2. 3. 'when the mountain ash blooms, it is good to sow wheat,' or p. 232 I (= 'Cures'), 30. 'vodka is drunk to cure a cold and to relieve a headache': the latter at least represents a rather international superstition.

The interesting description of the Cheremis religious movement *Kugu sorta* (pp. 320-337) totally lacks *ad rem* source references. It would be good to know the sources of the tale about the 'Pan-Finnic ideas,' etc., of certain 'Cheremis intellectuals' referred to on p. 337, since these things—at least here in Finland—have remained absolutely unknown.

PENTTI AALTO

**The Sherpa and the Snowman.** By Charles Stonor. London (Hollis & Carter), 1955. Pp. xii, 209, 37 illus. Price 18s.

**I22** Anthropologists will find Mr. Stonor's account of an expedition in search of the *yetti* or 'snowman' the most informative of the many travel books recently published on Nepal. The attempt to find a *yetti*, an animal about whose existence there can be little doubt, was unsuccessful, but while he was preparing for the arrival of the main party and in between several trips to the

## EUROPE

**Coal is our Life.** By N. Dennis, F. Henriques, and C. Slaughter. London (Eyre & Spottiswoode), 1956. Pp. 255. Price £1.5s.

**I23** After the usual diet, it is refreshing to read a report of social life in a modern community which is free from tables of correlations, which is well written and jargon-free, and which holds the interest of the reader. Reading *Coal is our Life* is not an unpleasant chore—keeping up with current work in the field—but an enjoyable experience.

The authors trace out the interrelationships between various aspects of the social life of Ashton, a Yorkshire mining town. They do not claim to have explored every aspect of the social life of this town of 14,000 inhabitants. Nor, as they point out, have they been able to give full attention to what they call the 'outside factors' which influence the life of the town. But they have succeeded in their modest aim, which was to make a specifically anthropological contribution to our understanding of the England of the mid-twentieth century; a contribution which other disciplines, with their emphasis on specific aspects of social life—the economic, the political, and the industrial—are not so well equipped to make.

Over 70 per cent. of the occupied males in the town work in the coal industry, 60 per cent. at local collieries. There is no other industry of any consequence in the locality and employment opportunities for women are few. Thus 'Ashton's families have a common fate determined by virtue of their similar relationship, through a wage-earning husband, to the coal industry.' By what may be called descriptive analysis—the weaving together of fact and interpretation—the authors trace out in detail the implications of this relationship for the miner at work, for Trade Unionism in the town, for leisure-time activities, and for family life.

high, uninhabited country believed to be the haunt of the *yetti*, the author spent many weeks in the villages of the Sherpas of Khumbu, and his observations among this remarkable people are well and vividly told. From his account emerges the picture of an economy extraordinarily well adapted to an inhospitable habitat, and capable of supporting cultural institutions of considerable complexity. Were the herding of yak and the cultivation of buckwheat and potatoes their only productive activities, the Sherpas would barely live on subsistence level, but their extensive trade with Tibet and the lower regions of Nepal enables them to augment their food with imported grain, to wear on festive occasions clothes made of Tibetan and even Chinese materials, and to find the means for the maintenance of monasteries and convents. Throughout the book Mr. Stonor gives us numerous interesting glimpses of the inhabitants of these institutions, and Chapter 13 is entirely devoted to an impressive description of a great festival in the Buddhist monastery of Gondah above Thami.

The author was not principally engaged in anthropological enquiries, and it is therefore not surprising that there are a few slips in his observations. Thus I cannot agree with his characterization of the Sherpas as lacking 'any positive, creative spark,' and consequently any art. It is not correct that all the frescoes in private chapels and temples are the work of Tibetans. Among the Sherpa lamas there are painters in no way inferior to the average Tibetan painter, and many of the frescoes in the monasteries of Khumbu and Solu are the product of local talent. But it is true, of course, that the religious art of the Sherpas, no less than the details of Buddhist ritual, have their roots in Tibetan lamaism. The particular painter resident in Khumjung mentioned on p. 102 is not, as the author believes, a Tibetan, nor indeed is he of Sherpa origin. His parents were Gurungs, and his acceptance into a Sherpa village community and marriage to a Sherpa girl of good class illustrate the unusual absorptive power of Sherpa society, in which innumerable immigrants from Tibet as well as isolated members of such ethnic groups as Gurungs, Chetris and Newars have been effortlessly naturalized.

The author's excellent photographs greatly add to the value of the book, which must be welcomed as a useful corrective to the many mountaineering books in which Sherpas only figure as bearers of burdens, and never as the creators of a highly specialized Himalayan culture.

C. VON FÜRER-HAIMENDORF

The chapters on the miner at work contain clear and detailed descriptions of the work of getting coal. I know of none elsewhere so valuable as these. But the analysis is marred by a too doctrinaire approach to industrial conflict. Stress is rightly placed upon the fact that the working conditions of the miner give occasion every day for conflict between worker and manager. But too little emphasis is given to the factors which encourage co-operation. And at times working-class solidarity is stated as a stubborn fact of life in the face of contradictory evidence in the book itself. But perhaps the emphasis on conflict is itself valuable as a corrective to the approach of the Human Relations School.

On leisure and family life the book is excellent. There is a valuable analysis of the function of gambling, which is enlivened by many amusing and telling anecdotes. I particularly liked the following reported conversation between two miners:

A. 'I just missed the Treble Chance last week. Just one match let me down.'

B. 'I don't know why you bother at all. I never do. You have no chance at all.'

A. 'I've a better chance than you, at any rate, if you don't bloody well send them in at all.'

*Coal is our Life* does not patronize the miner, it does not claim to have discovered the answers to the troubles of the coal industry, and it does not moralize. In short, it is a modest and honest piece of work. Perhaps its major shortcoming is its lack of any attempt to relate the findings at Ashton to the work of sociologists in communities in this country and America, and to examine the implications for sociological theory.

*Coal is our Life* is in itself a plea for a shift of emphasis in sociological studies from isolated social problems and aspects of social life to the study of the interconnexions between social phenomena. A mining community was an excellent place to start.

TOM LUPTON

**Masken in Mitteleuropa.** By L. Schmidt. *Sonderschriften des Vereines für Volkskunde in Wien I.* Vienna, 1955. Pp. v, 253, 27 plates, 3 text figs., 2 maps

**I24**

Switzerland and Austria, which harbour the most important collections of Central European masks, have also been leading in the study of their origin and meaning, distribution and customary usage. After surveying the various schools of thought, Professor Schmidt suggests that an investigation of the masks at various given periods would greatly further the 'Masken-Forschung.'

#### OCEANIA

**The Botany of Cook's Voyages.** By Elmer Drew Merrill. Waltham, Mass. (*Chronica Botanica Co.*) (U.K. agents: Wm. Dawson), 1954. Pp. 163-384, *Chronica Botanica*, Vol. XIV, No. 5/6. Price \$4.75

The late Professor E. D. Merrill, a very distinguished American botanist, died early this year. This, his last book, is the product of a re-examination of the botanical collections made during Cook's voyages by Banks, Solander and the two Forsters, father and son. Professor Merrill shows that the significance of these collections, which are now widely scattered, has never been fully appreciated. Some of the material has not been published. All the collectors mentioned were competent botanists, and they collected cultivated plants, weeds and 'wild' plants with equal assiduity. Detailed examination of their material leads Professor Merrill to the conclusion that 'the botanical evidence clearly indicates that, as of 1769, all the cultigens in the Pacific Islands, with possibly one exception, and all the more numerous weed species, with possibly two or three exceptions, were brought in purposely or inadvertently from the west by the early people who occupied the Polynesian triangle' (p. 191). He excepts Guam from this generalization, since many Mexican species were introduced by the Spanish galleons which maintained a regular if infrequent service from 1565 until 1815. He adds that '... there is no valid evidence that any of the important cultigens of American origin became universally distributed in both hemispheres in pre-Columbian or pre-Magellan times' (p. 192: author's italics in both cases).

Professor Merrill's work is highly relevant to anthropological studies in two main respects. He discusses with authority the origin and diffusion of important food plants, with critical examination of the various theories and the most recent evidence on what is still in many cases a subject of controversy; and he dissects both the conclusions and the methods of certain botanical and anthropological writers. It may be useful to summarize his conclusions in these fields.

1. Only the gourd (*Lagenaria*) was definitely common to America and the Old World in pre-Columbian times. It is probably of African origin and reached America by floating; in controlled tests gourds which had been afloat in sea water for nearly two years still contained viable seeds. Heyerdahl's distinction between the bottle gourd and the giant gourd is not considered valid.

2. The sweet potato may be a hybrid of African origin which reached America in pre-Columbian times. Professor Merrill seems at one point to incline to this view and at another to favour its American origin. It was not found by Bird among the vegetable remains at Huaca Prieta in Peru. Firm evidence is still lacking.

3. The coconut is of Old-World origin and reached the west coast of America by 1520. The wide distribution of the family to which it belongs (many members of which are native to America) is probably to be explained by the former fertility and extension of the Antarctic continent.

4. Maize is of American origin; it did not originate in Assam as has been suggested. The primitive Assamese types are derived from the popcorns of eastern Brazil. Their occurrence in Assam is to be explained by the Portuguese trade route from Lisbon to the bulge of eastern Brazil and thence round the Cape of Good Hope to Goa. This route was extensively used from 1500 onwards (i.e. several

In Central Europe, among the hunters, stag and bear masks were outstanding and among the herdsmen the masks of bull, ram and goats. The first human masks may be contemporary with the fertility idols of the planters. The death mask from Klein-Glein, in Styria (seventh century B.C.) leads to short discussions of metal masks from China to Mycenae and of the different materials masks were made of, e.g. clay and wood. Horse masks are, of course, late-comers, and probably date back to classical antiquity; they have only been found in the non-mountainous border countries.

Other contributions deal with the devil's mask in folkplays, with masks in Bavaria, Switzerland, Slovenia and Poland. The following references may be found useful: to the Cornish play *The Creation of the World* (pp. 90-1), hobby horses (p. 110), English masquerades (pp. 124, 127) and the Green Man (p. 207).

E. ETTLINGER

decades before the opening of the Spanish route from Mexico to the Philippines). Before long the Portuguese were pushing eastward; Macao was founded in 1557, and from there they traded in China, Formosa and Japan. Professor Merrill repeatedly emphasizes the importance of this route from Brazil to Goa for the early distribution of plants; it seems to have been as largely neglected or underestimated by botanists as by anthropologists.

5. The theory that tobacco was introduced to New Guinea in pre-Magellanic times launches Professor Merrill into an interesting discussion of varieties of the tobacco plant and of the names for tobacco in Latin America, New Guinea and South-East Asia. He concludes that the only species cultivated in New Guinea is *Nicotiana tabacum*, an American hybrid; that there is no native species in New Guinea; and that its distribution is certainly post-Columbian. He agrees in the main with the conclusions of Haddon, for whose work in this field he has a high regard.

6. Professor Merrill emphasizes that agriculture and stock-raising in the Old and New Worlds were based on completely different plants and animals (the dog being the only exception), and regards this as unanswerable evidence for the independent origin of these practices in the two hemispheres.

Of particular relevance to current anthropological controversies is the chapter called 'A consideration of certain "Authorities".' The authorities are mostly botanists who have been concerned with plant-distributional studies—F. B. H. Brown, G. F. Carter, C. Sauer and (passim) O. F. Cook in particular—and whose work has influenced anthropological thought. His strictures on their methods and conclusions, pungently expressed and convincingly documented, must come as a surprise to those of other disciplines who have accepted their views as sound and well based. The only non-botanist who has the misfortune to attract Professor Merrill's attention (except for some passing references) in this chapter is Thor Heyerdahl. The demolition of his 'authorities' is merciless, but otherwise Professor Merrill treats him comparatively gently, apparently regarding him as one who sinned in ignorance. The verdict is nevertheless devastating. '... Heyerdahl's phytogeographic views are not sound, simply because he did not know the subject and was hence incompetent to judge the actual value of the work of those on whom he depended for his evidence' (p. 222). 'His 1952 bibliography of more than a thousand titles is impressive. It is also obvious that articles and volumes, covering a vast range of subject matter, were not only read but that they were intensively studied in a search for statements which would help "prove" what this courageous explorer believed and wished to prove. Whatever he thought supported his theory was too quickly accepted as true evidence' (p. 264). Professor Merrill might have added that statements were quoted out of their context in a way which is sometimes highly misleading. 'The tetraploid cotton theory of Hutchinson, Silow and Stephens . . . has never been and scarcely can be proved. Nevertheless, Heyerdahl makes much of it' (p. 265). 'At least four of the taxa mentioned by Heyerdahl as "South American" elements for Easter Island are endemic species, i.e., known only from Easter Island!' (p. 269; author's italics). 'It is clear that there is no botanical evidence to support the claims made by Heyerdahl in 1952.' 'His case seems, to me, to show beautifully how

the human mind, once decided, can practice self-deception' (p. 270). The suspicions which must have been felt by anyone who has carefully checked a random sample of Heyerdahl's ethnological evidence are here confirmed. It would be interesting now to hear the views of a physical anthropologist on Heyerdahl's evidence from blood groups, hair and skin colour.

*The Botany of Cook's Voyages* is a rich source of fruitful comment and out-of-the-way information. The spread of weeds, Professor Merrill shows, was an inevitable result of the Polynesian practice of carrying plants in soil on their voyages. Some, such as the bread-fruit which sets no seed, could have been spread in no other way. The need for the early European explorers to carry a proportion of their meat alive, and hence to cut hay at every convenient landfall, had the same result. But what is one to make of the pineapple in a wall-painting at Pompeii? It poses two difficulties: the chronological one and the fact that the pineapple will not flourish in the Mediterranean climate. Professor Merrill suspects that it is the work of a nineteenth-century restorer. It should be possible to settle the point by analysing the pigments.

It must be said that this book is not easy reading, since the argument occasionally seems disconnected and there is a tendency to diffuseness and repetition. Criticism is disarmed by Professor Merrill's note on 'The Author's Personal Handicaps,' in which he

states that because of his advanced age this would be his last book. Nevertheless its pungency, clear thinking and sound sense make it not only of great value but most refreshing. Anyone with a hankering to prove a preconceived theory should read it and be chastened.

B. A. L. CRANSTONE

**Love in the South Seas.** By Bengt Danielsson, translated by F. H. Lyon, London (Allen & Unwin), 1956. Pp. 240, 15 plates. Price 16s.

**I26**

A popular book such as this on the sexual practices and attitudes of the Polynesians (the author does not concern himself with Melanesia) has a useful function to perform in correcting common misconceptions. Unfortunately the title and the dust cover may attract the wrong type of reader, but such readers will be disappointed. Most of its value derives from the author's personal knowledge of Polynesians; but he has read widely too, and his book seems generally sound though marred by too much facile psychology and superficial moralizing. Its anthropological value is variable. It provides some interesting light on present-day conditions and is quite good on the Aroio society and on sexual life generally before European contact—but what is one to make of an account of bark-cloth manufacture which does not mention beating? There is a good bibliography of over 100 titles.

B. A. L. CRANSTONE

## CORRESPONDENCE

**Tiv Hairdressing: A Change in Custom. With two text figures**

**I27** Sir,—We have, in comparing notes and impressions about the Tiv of central Nigeria, come upon a change in custom of some interest. Downes found, in the early nineteen-thirties, that all Tiv women, at least in the south-east, had their hair dressed by men. Bohannan found, in the late nineteen-forties and early fifties, that all women's hairdressing was done by



FIG. 1. TIV HAIRDRESSING IN 1933

Photograph: R. M. Downes

women. Our initial reaction to the discovery of this difference was to consider one another 'wrong.' Fortunately, we both had photographs to document our statements. Fig. 1 was taken by Downes in Turan in 1933; fig. 2 was taken by Bohannan in Shangev Mbashaya in 1950. This change in division of labour was not



FIG. 2. TIV HAIRDRESSING IN 1950

Photograph: P. Bohannan

discovered by Bohannan in the field—probably because it never occurred to him to ask specifically, and the Tiv considered it of no importance.

R. M. DOWNES  
PAUL BOHANNAN

**Indians in Africa.** Cf. MAN, 1956, 89

**I28** Sir,—With reference to Dr. Margaret Murray's letter in your June issue, no argument as to the whereabouts of Punt can safely be based on the hunting leopard (cheetah). The species is widely spread in Africa, as well as in India, Arabia, Syria and Asia Minor.  
*New Radnor*

J. H. HUTTON





(a) Abaji scars: a 'lumpy face'



(b) Mkali scars superimposed on nail scars



(c) Women's back scarification



(d) Handsome calves are emphasized by scars

**TIV SCARIFICATION**

Photographs: Dr. P. J. Bohannan

# BEAUTY AND SCARIFICATION AMONGST THE TIV\*

by

PAUL BOHANNAN, M.A., D.PHIL.

Institute of Social Anthropology, University of Oxford

**129** Europeans seldom count scarification and tattooing among the fine arts. Amongst some African tribes, however, there is an aesthetic of body decoration which should be examined if we are to understand African artistic canons. Tiv, the large pagan tribe of the Benue Valley in Nigeria, are heavily scarred, and have a definite and largely overt aesthetic of physical beauty, including scarification.

A favourite Tiv *bon mot* is *kasev hemba kongoron kwagh*; it is one of those multiple puns in which they delight. It means both 'women rub more things' and 'women are more smooth'; it means that women spend a lot of time with pomades and poultices for the sake of their appearance, and has direct sexual connotations. The Tiv aesthetic of physical beauty is explicitly built on the assumption that one should make oneself attractive, and that the proof rests in being looked at. Its implementation calls for (1) oiling or colouring the skin, (2) dressing up, (3) chipping the teeth in an unusual or pleasing way and (4) incising the skin.

Tiv oil their bodies with palm oil, castor oil, vaseline, or, occasionally, groundnut oil. The resultant shining quality is highly prized: it is said of a person with a glistening skin, 'he glows' (*a wanger yum*). The same word is used of the sun or the headlights of an automobile. The impersonal form of the same expression, *i wanger*, means to be light and can be said of the day, of a kerosene pressure lamp or of an idea.

One of the most effective ways of glowing is to rub the skin with camwood, a red wood which Tiv import from the forests to their south. Camwood is ground and made into a paste with either water or oil, and is used as a cosmetic. It is also involved in ritual: the notion of lightening or causing to glow joins the profane and sacred worlds. The bride and groom, especially if it be the first marriage for both, are smeared with camwood so that they glow; they may wear it for several weeks. A new-born child has camwood smeared on his head. Corpses are smeared with camwood; this 'lightens' them, but also (Tiv are practical people) absorbs the liquids from the corpse and keeps down odour. Whenever a person is put into special contact with a fetish force or *akombo*, camwood is necessary to the ceremony. The greatest protecting force in Tivland, *swem*, has camwood as one of its ingredients. Yet many people smear camwood on themselves merely because they think it attractive. Occasionally, if camwood is unavailable, yellow ochre may be substituted for cosmetic purposes. Today talcum powder is a cosmetic, and the same phrase is heard—'he glows.' The whole notion of lightening by making oneself smooth, attractive and sacred is important to Tiv in their religious and personal lives.

\* With Plate I and eight text figures

Tiv say that it is possible to make oneself very light by means which are horrible and nefarious: if one smears one's body with human fat, one becomes irresistible. There are said to be men who deal in human fat: they come, with sacks across their shoulders, and sit menacingly at the side of the market; their mark is a piece of a certain grass hanging from their lips. A prospective buyer approaches circumspectly; the haggling does not take place in the market, but arrangements are made to meet for a special 'market held at night' (*kaso tugh*). We can see that Tiv are expressing metaphorically their belief that if one man achieves too much worldly success—including too much beauty—he must have got it at the expense of another.

There are other, less drastic, magical or medicinal means of 'glowing.' Tiv wear charms sewn into small leather bags or bracelets, some of which make one invisible or turn bullets to one side, but many more of which protect one against witches or make one attractive in general and sexually attractive in particular.

The first notion of physical beauty, then, is that the body must 'glow' or *wanger*. It is also the point of much ritual. The word means to be beautiful, to be clear, and to be in a satisfactory ritual state.

Besides being made to glow, the body must be adorned or decorated. The Tiv word for decorate is *wuha*. It means to make more pleasing anything which already glows or *wanger*. The glowing body is adorned with clothing and jewellery. Usually translated 'to dress,' it really means 'to dress up.' *Swem*, the greatest power or natural force, which itself glows, is 'dressed up' whenever its representational ingredients are put together for a ritual purpose.

One of the things which children do when they 'dress up' is to paint designs on their faces with a vegetable juice called *mar*. When they get a bit older, they begin conscientiously to 'try' various designs to see which ones fit their particular faces. There is an intricate vocabulary of names for the various sorts of facial marks which may be made with *mar*. Schoolboys write words and numbers on one another's faces; often nonsense syllables are written for the sake of the design. The number '5' was a favourite.

Tiv use other cosmetics as well. They dye their fingernails and palms with henna, keeping gourds full of moistened crushed henna fastened over their wrists for several days at a time. They also use antimony or galena—the mineral which they call *tojii*—to paint eyelids, nostrils and sometimes lips, or to blacken eyebrows. Men sometimes blacken their moustaches with *tojii*.

Another type of facial decoration is *shase*, a tree which grows in Tivland (*Upaca guineensis*). If you take a pointed twig of this tree, it leaves a spot wherever the skin is pricked; the spot stays for two or three months and then

gradually disappears. *Ishase* scars are white, and are very effective on a black skin; their most common use is as a 'trial run' for more permanent scarification. Every effort is made to get patterns which emphasize the best points of one's face.

One of the most important requisites for beauty is that a person be scarred (*gber*, literally 'cut'). Akiga records a

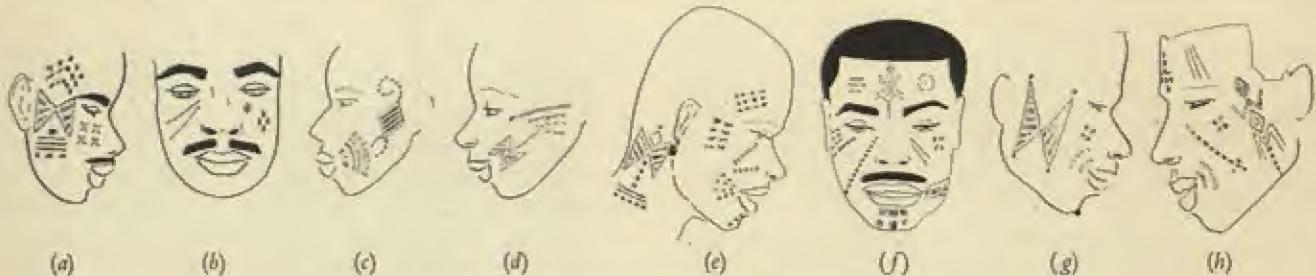


FIG. 1. 'NAIL' SCARIFICATION PATTERN—ALL MALE EXCEPT (e)

legend that Tiv were originally unmarked and took up scarification to distinguish themselves from other tribes.<sup>1</sup> Tiv markings are very characteristic, but my informants denied that they were 'tribal marks' with which they were familiar amongst Ibo and Yoruba. Rather, scarification style changes from one generation to the next. Though one's scars may mark one's generation, they do not mark one's lineage.

Akiga has, with high spirits, described the struggle between the 'lumpy faces' and the 'nail boys' which took place when scarification styles changed in the nineteen-thirties. Although he has, in typical Tiv fashion, overstated his point in the interest of humour, it is true that young women's preference for young instead of old men is sometimes expressed in a fondness for new types of facial marking. There are four 'generations' of scarification types to be found in Tivland today. The oldest of these is called *ishondu*; they are seen occasionally on very old people, and are sometimes done today, as an 'old-fashioned' gesture, on young men. They are flat, shiny scars along the arms and down the back. They are followed by *abaji*, Akiga's 'lumps' (see Plate I a). *Abaji* are made by means of a hook—today a fish hook with the barb filed off. Some skin is hooked, lifted, and then cut away with a Tiv razor; a styptic agent (*alufi*) is applied to stop bleeding, and charcoal and perhaps indigo is rubbed into the wounds. *Abaji* are most commonly cut around the eyes. Akiga says that there were three or five, but most of the people I knew had six. During the time when I was in Tivland (1949–53) *abaji* markings were found on almost all men and many women above the age of 35 or so. Younger men had a new sort of marking called 'nail' (*kusa*) after the instrument with which they were made. Nail scars are flat and very difficult to photograph. Fig. 1 is seven sketches of 'nail boys' and one woman marked with nail scars.

Some years after the introduction of nail scars, another sort of scar called *mkali* became fashionable. *Mkali*<sup>2</sup> are very deep scars cut with a razor and coloured black with charcoal. It is possible to put *mkali* over nail markings much more effectively than it is to put either over 'lumps.' One

often sees all combinations, however. Fig. 2 shows one example of combined lumps and nail markings, and five examples of nail markings and *mkali* marks. Plate I b shows *mkali* marks superimposed on nail markings.

Tiv definitely associate these different types of scars with different ages of men. I have heard young men accused of putting scars of the older generation on their faces in order

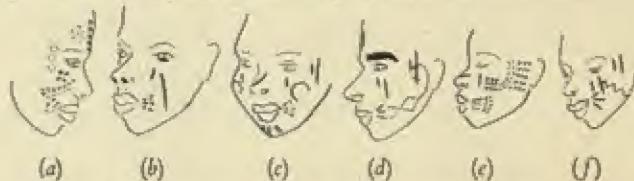


FIG. 2. SCARIFICATIONS OF MIXED TYPES

'Nail' marks are mainly decorative; both the *abaji* and *mkali* marks, however, actually alter the planes of the face. They are cut very deep, and their effect is on the fall of the shadows on the face. Prominent cheeks, for example, can be made more prominent by doubling the shadows cast upon them. A nose can be made longer—or shorter—by use of a deep mark. The ultimate purpose of all scarification, Tiv insist, is to make themselves more attractive.

Complete scarification may take decades. It begins at the age of 13 or 14 and may go on until one reaches 40 or 45. I knew one woman of 35 who had saved an empty space on her forehead for the time she would leave her second husband. She had now done so, and was casting about for a design to fill the space. She spent days trying designs in ashes and gazing into a mirror. She insisted that I offer a suggestion; I was relieved when she decided against it.

Scarification in the nail style is suitable for making designs on various parts of the body. Men decorate their chests with geometric designs (fig. 3a), to which they may add animals or birds, like the chameleons of fig. 3b. They also often cut designs on their arms; representative arm designs are seen in fig. 4; I have also seen knife scabbards of the sort usually worn on the arm, scarified in a naturalistic position.

Women have scars put on back and legs in preference to chest and arms. A girl who is lucky enough to be born

with 'good' legs (full calves and prominent heels) will probably call attention to them by having a design put on them, and by wearing a string of white or coloured beads just below the knee. Most of these designs are cut with a sharpened nail or a razor, and the resultant wounds made into raised scars by rubbing charcoal or camwood into

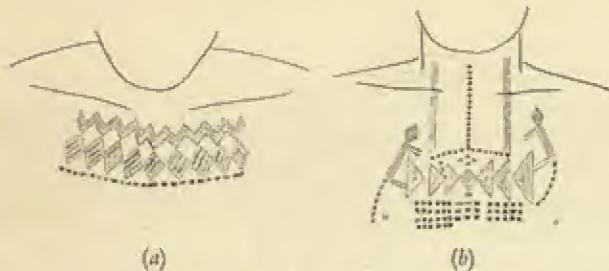


FIG. 3. MEN'S CHEST SCARS

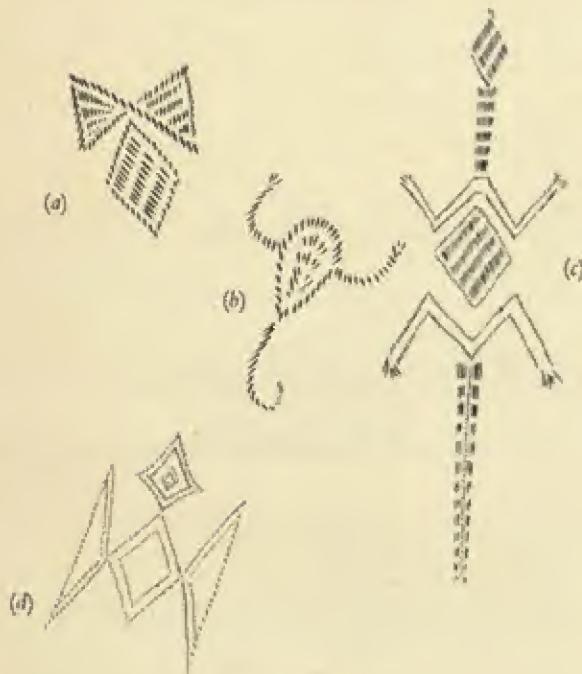


FIG. 4. ARM SCARS

them. Usually the design is one which the Tiv call 'fringe,' after the edges which are left on a piece of cloth when it comes off the loom. A really handsome set of leg scars will be famous for many miles around (see Plate I d).

Somewhat more common are the scars which are put on women's backs with many fine razor or nail cuts (Plate I c and fig. 5). These scars usually start on the neck, just below the hairline, and come about halfway down the back. The number of artists who do back scars is limited; after one has studied them for a few days, one learns to recognize the personal styles. These artists are often circumcisors as well.

Scarification designs are common to both sexes. They consist of geometrical designs or are representations of the swallow, the water monitor, the scorpion, the fish (*ishu*) or occasionally the chameleon.

Tiv admire the swallow above all birds. They build small platforms into the roofs of their reception huts especially for swallows to nest on, and say that swallows will not live with a man who has witchcraft substance on his heart. They greatly admire the swoop with which the swallow dives from the uppermost point of the roof, out

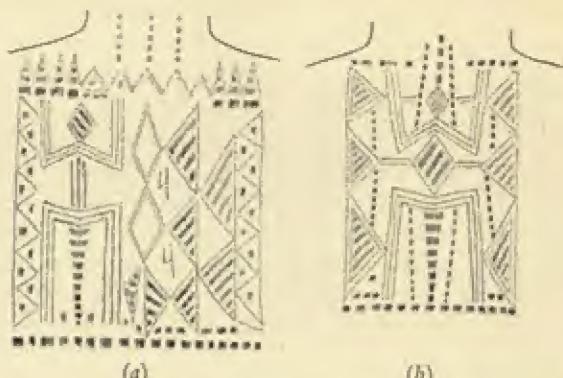


FIG. 5. WOMEN'S BACK SCARS

under the thatch and into the free air. I have often been asked, 'Wouldn't you like to be able to do that?' The swallow pattern is a conventionalized one (fig. 8a); it is found over everything which Tiv do—their carved calabashes, scarification, wall paintings. Swallows are signs of agility and freedom, and more particularly of a good heart (see figs. 1d, g, 4d, 5a).

The scorpion (*iye*) is admired by Tiv for reasons which we should not consider admirable. It never gives away its position before it strikes; it is always present, always dangerous if disturbed, and most difficult to see. The basic scorpion design (fig. 8b) is representational, and is often found tattooed on arms and faces, occasionally on a man's chest (see figs. 1c, 2d, 4b). It is painted on house interiors. I have never seen a scorpion among a woman's scars.

The chameleon is the least common animal design. Tiv refuse to kill chameleons; the only reason I could ever get for their refusal to do so is that the beast can't help being so ugly and repulsive.

Lizards—actually, water monitors—are vastly admired by Tiv and are often found scarified on the arm; they also play an important part in the back-scarification of women. I do not wholly understand the symbolism of the water monitor. In the folktales, water monitor is a fool and is often the dupe of the hare. It is in part, I think, a regard for the stylization of diamonds and lines which makes it popular (figs. 1f, h, 4c, 5a, b).

The fish design (fig. 8c) is not to be confused with the swallow or with the mudfish design of belly scars to be mentioned below. The side of the neck is a favourite place for a fish (figs. 1e and 6), but it may be found on the face (fig. 1a) or on other parts of the body.

The most characteristic scars found on Tiv are those on the bellies of women (see *Akiga's Story*, plate facing p. 43). The belly design is called a 'catfish' (*ndiar*). Abraham (*A Dictionary of the Tiv Language*) says that it is *idiar*, which means sexual lust. I have no doubt that Tiv told Captain

Abraham that it was the same word—they often make puns on it. Unmarried women with particularly good scars are teased with this pun, and it is a favourite joke that the design of the tail of the fish is finished off with the clitoris. All, however, tell me that the design symbolizes the catfish or mudfish.

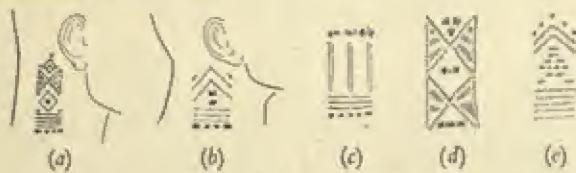


FIG. 6. NECK SCARS

The head of the fish is represented by a knot of scar between the breasts. It has a long neck, and fins which are represented by the wing-like extensions of the design on both sides of the navel. The only time I ever saw these scars cut, they were cut on two girls just past puberty, and on a young woman who had been married for three or four months, whose husband wanted her to be scarred. Like circumcision, they were done early in the morning, before the sun gets hot—blood, like oil, is thicker in the

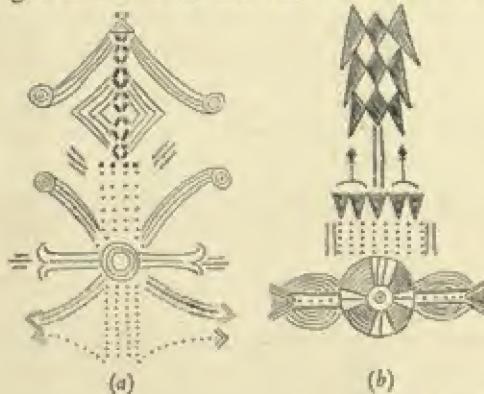


FIG. 7. WOMEN'S BELLY SCARS

morning. The operation is performed with the girl lying back in a Tiv chair (see MAN, 1954, 2, Plate A), with her back arched. The design is drawn in charcoal before it is cut; the lines are done with a razor, the dots with a hook and razor by a method similar to that used for *abaji* facial scars. Charcoal is rubbed into the wounds.

Like most activities amongst Tiv, scarification takes place in the middle of the compound; anyone who likes may watch it. The operator is encouraged and advised; women remind one another how much it hurts; young men may give encouragement to their girl friends.

Mudfish scars on women's bellies are said to promote fertility, but if you ask Tiv in just what way this works, they say that the scars are tender for some years after they are cut, and are therefore erogenous, and that a woman who has them will demand more sexual attention than one without them, and hence is more likely to have children. Tiv are, as Frobenius put it, 'ein sehr praktisch und unaberglaubliches Volk.'

The mudfish patterns are of two basic sorts: they are called the 'Okpoto fish,' marked by a triangular motif

(fig. 7b) and the 'Tiv fish' which is more cursive in design (fig. 7c, d). A 'swallow' may be added many years after the fish design; this swallow is a different stylization from that used elsewhere on the body. It usually comprises long, swooping double or treble lines which more or less centre on the navel. Some women told me that these were put on after a child or two had been born in order to keep the skin of the belly firm (fig. 7a).

The only other form of body decoration to be mentioned is tooth-chipping, which is fast dying out. The only tooth-chipper that I knew was a carpenter who worked with a chisel, file and pinchers from his tool kit. Tooth-chipping hurts—more, Tiv tell me, than scarification. When a man has his teeth chipped, it is always because his girl friend tells him he 'looks like a gopher' or 'like a crab,' and he brings witnesses to report back to her that he withstood the pain without flinching. Tiv say sometimes that having the teeth chipped helps one to learn languages. The two front teeth may be removed or pointed; a triangle may be cut into them or an arc taken out.

The purpose of tooth-chipping is, again, physical beauty. The effect on the face is always taken into consideration.

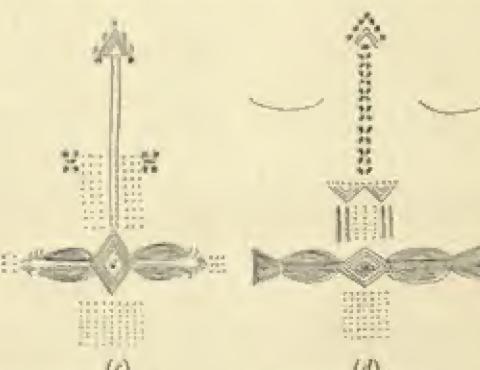


FIG. 8. SWALLOW, SCORPION AND FISH PATTERNS

having them knocked out, she had a nick put in each of them which gave the effect of her having four narrow teeth where the two broad ones actually were. One always looked at her, then, when she smiled, looked back at her again to be sure that one had seen correctly. I was told by someone else that her tooth operation had been successful because now everybody looked at her twice. She had, indeed, *wanger*; she glowed. The proof was that people stared at her.

Tiv do not associate tooth-chipping with cannibalism, and indeed they think it very funny that anyone should.

Tiv soldiers in Egypt during the war discovered that some people were afraid of them, saying that they were cannibals because of their chipped teeth. They told me that they used grimaces to advantage in haggling over prices in Egyptian markets.

The aesthetic of beauty, in so far as it is represented by scarification and chipping of the teeth, is involved with pain. I once asked a group of Tiv with whom I was discussing scarification whether it was not exceedingly painful. They turned on me as if I had missed the entire point—as, indeed, I had. 'Of course,' one of them said, 'of course it is painful. What girl would look at a man if his scars had not cost him pain?' The effort to 'glow' must be obvious; the effort to be dressed up must involve expense and

trouble; scarification, one of the finest of decorations, is paid for in pain. The pain is the proof positive that decoration is an unselfish act, and that it is done to give pleasure to others as well as oneself. The probable pain is the measure by which Tiv regard scarification; a second measure is the good taste with which the scars fit the face and augment the personality.

#### Notes

<sup>1</sup> This and subsequent references to Akiga are from *Akiga's Story*, translated by Rupert East, London, 1939, pp. 38–49.

<sup>2</sup> Akiga gives this word as *nkali* and derives it from the Jukun town of Wukari. I heard it *mkali*, and none of my informants made this derivation.

## A NOTE ON TANGU DREAMS

by

KENELM O. L. BURRIDGE

**I30** Some comments on the significance that dreams have for Tangu have been made by me elsewhere<sup>1</sup>; and though it was not difficult to obtain information about dreams, collecting dreams themselves was not easy since they are private and secret communications to the individual. Nevertheless, the examples provided hereunder constitute a fair sample and will suffice to indicate the kind of dreams that Tangu have, and their range.

The great majority of dreams concern hunting, especially killing or trapping a pig:

One night *K* dreamed that he would trap a pig. Next morning when he awoke he repaired to his trap and found a pig there.

*M* dreamed that he went up a tall betel palm to pluck some betel nuts. When he came down he chewed betel, pepper leaves, and lime. Through the heat of the lime it occurred to him that he had trapped a pig. Next morning when he awoke he went to his trap in the bush and found a pig there.

*N*'s mother had a dream in which she was digging a pig trap in the pouring rain. But something told her to go on digging despite the rain. Next morning when she awoke, she dug a large hole and placed some branches and leaves over it. The following morning, after a night of rain, a pig was found in the hole.

One night *Kw* was awakened by a scratching noise on the side of his hut. Thinking that it might be a sorcerer he crept out of the hut to look. There was nothing there. But the scratching noise continued and it seemed to *Kw* that it was some kind of exhortation. Next morning he did not go out to his garden but stayed in the village. During the day a stranger passed through the village and conversed with *Kw*. He mentioned that he had been digging a hole for a pig which he had tracked in his own hunting bush far away. Immediately, the scratching noise which *Kw* had been hearing all day stopped. Next morning *Kw* went out to dig a hole for a pig and the following day a pig fell into it.

*R* made a trap for a pig. During the night he dreamed that he saw a pig entering the trap. Next morning when he went to have a look at his trap he found a pig there.

Although such experiences may be described as omens or 'hunches,' and even if some dreams are invented after the event, it only makes the connexion between the pragmatic techniques required to succeed in the hunt and dreams more obvious.

Other dreams concern sorcerers:

After a dance—when sorcerers are considered to be particularly active—*N* shouted in his sleep. He said later that he had been dreaming that as he came into his hut he espied a sorcerer hiding in the back, and so shouted from fear.

*P* dreamed that he was being throttled by a sorcerer. Next morning he was so ill with a stomach complaint that he could hardly stand up.

*M* was sleeping. He dreamed that he got up beside his second wife and they both saw someone creeping under the hut. They thought that it must be a sorcerer, but then the wife suggested that it might be *M*'s elder brother (who is dead). Next morning *M* and his wife compared notes. They had both heard a noise, and they both thought that it must have been a sorcerer after their daughter.

Some dreams concerned myself:

*M* dreamed that his elder brother (deceased), he and I were seated in the village dancing space. I was in the middle. There were numerous good things about—food, a ship, pipes, cloth, soap. The elder brother then detached himself and approached with outstretched hand. *M* woke up.

*D* dreamed that he, his deceased father and I were sitting down together. The father was in the middle. The father pointed out to *D* that I did not *rousim* (shoo him off) as other white men did; that I could teach them much; that the missionary was not the only schoolmaster in the world. Then I gave them meat. The father said that *D* was to take care of me and do whatever I asked them for 'I had their number in a very large book.' *D* woke up.

The suggestions here are obvious and need no labouring. Just as obvious are the dreams that were related to me on the approach of an administrative patrol:

*M* had a very bad dream. He had come into the village and found it horrible. He was shocked. Then, as he looked around

him it changed. The village seemed to him to be very good. His (deceased) father came into the village and looked on it approvingly.

*L* dreamed that he came into the village and found it very dirty. He was shocked because if the patrol came and found it thus they would all be beaten up. Then it became beautiful and clean.

#### Other dreams reflect the parting of kinsfolk:

*G* dreamed that she went back to her natal settlement and there her (dead) father and mother cooked some food for her and gave it to her to eat.

*D* dreamed that his father and mother came to him and said 'Is this your house?' On his answering in the affirmative, his parents said, 'Well, you stay here, and we will remain where we are.'

*H* dreamed that his younger brother was coming back to Tangu after his spell of contract labour somewhere on the coast. He brought pawpaws with him—one for me. *H* said later that this dream meant that his brother would return soon.

#### At first glance some dreams are amusing:

*M* dreamed that he was wearing a tiny pair of spectacles—like those of the missionary, only much smaller. When he put them on his nose he could see everything in great detail and how good everything was.

*R*, my cookboy, dreamed that he was shaving the back of my neck. By some misfortune he cut it.

#### Other dreams are more ominous:

*U* was very ill. She dreamed that her mother (deceased) came to her and scolded her for taking some of her full brother's yams without first asking him. The mother was very angry. *U* protested 'Oh, but I only took one.' Thereupon her mother beat her over the thigh with a chunk of firewood. Next morning when she awoke, *U* found a swelling in her thigh. She became very ill, and the following day she died.

*G* dreamed that he went hunting a pig with a dead ancestor. But as they came near to a stream the ancestor speared *G* who died there.

*K* dreamed that he was being hit and beaten up by a man with an adze whose face he could not see.

*B* dreamed that he was chasing a girl *N* who had already been marked down as the wife of a brother of his.

*F* said that he had had a bad dream. He dreamed that he was being thrashed by his classificatory sister. Then he woke up.

*W* was mourning the death of her brother. She dreamed that her brother made himself a breechclout and put it on the sand while he went into a stream for a bathe. He called out to her from the stream, 'Where is my breechclout?' She did not know. He came ashore and instead of the breechclout he saw a snake. The snake killed him.

*G* said that if you dream you fight a man you had better stay at home. If you go into the bush you will surely come across a pig. You will say 'Oho! Now for a meal!' But your spear will miss and the pig will bite you in the legs and throw you, then bite your forehead, then chew your eyes . . .

One may point out that dreams in Tangu are very much concerned with dead as well as living kinsfolk, that feelings of guilt enforce dreams or are associated with them, and that, with the same qualification, desires beget dreams. In addition, the imperative contained within a dream may be used as a reinforcement when attempting to persuade another person to a particular line of action or point of view. That is, even when reflective or 'explanatory,' dreams are also prophetic. When a man dreams that he has trapped a pig he visits the trap in high expectation. When a man dreams that his brother is going to return from a spell of contract labour, the dream means that the

brother *will* return shortly. Nor does this notion exclude the possibility that the dream may be a trick; that it may be a deception.<sup>2</sup>

There is a distinction between these everyday dreams, and the dream that triggers a Cargo cult—even though the general notions concerning dreams appear to remain constant. The cult dream is made public. It is not a private communication to the dreamer. In the cult situation the dreamer appears as a vehicle merely: he publicizes a dream which does not concern his own luck in the hunt, or only his private desires; it concerns the whole community. Such a dream is not only unusual, but on the analogy of the private dream which contains an imperative, it would seem to contain some compulsion to make the dream public. For if the dream has a public relevance it follows that it is one which each or any of the community *might* have dreamt—the dreamer is the vehicle of a 'collective dream,' a communication for the benefit of all and not for the individual alone. That is, it is a directive for community action. The fact that the directives may fail in gaining the ends does not vitiate the force or validity of the directive itself, for Tangu know that there is always the possibility of the dream being a deception. The problem then becomes one of deciding whether the dream is just such a deception or not. It is always worthwhile visiting a trap, for the failure to capture a pig means only a walk home. That is, the consequences of a deception are hardly sufficient to weaken the force of the imperative contained in the next dream. The dream as directive may still emerge as an axiom even though a particular dream may be a deception.

How about the cult dream—if it succeeds will it go all the way and preserve them from administrative rifles? If it fails some kind of retribution is sure. If it is a trick it is a pretty nasty one, for unlike other deceptive dreams the consequences are dire.<sup>3</sup> Tangu cannot remember whether they experienced 'collective dreams' such as the cult dream before the Europeans came to New Guinea. They say now of their cult activities that it was a 'very big trick,' and that they were foolish to listen to the dreamer—look at what happened! One might say that Tangu are forced either to make an antecedent distinction between one kind of dream and another—thus retaining a helpful technique of going about their affairs at the expense of their own (logical) notions concerning dreams, or to abandon these notions altogether. In fact, however, the real distinction appears to be related to consequences. The dream is worth acting on if the probable or foreseeable consequences of a deception are acceptable. Can such an idea find a place in any scheme of *a priori* moral axioms?

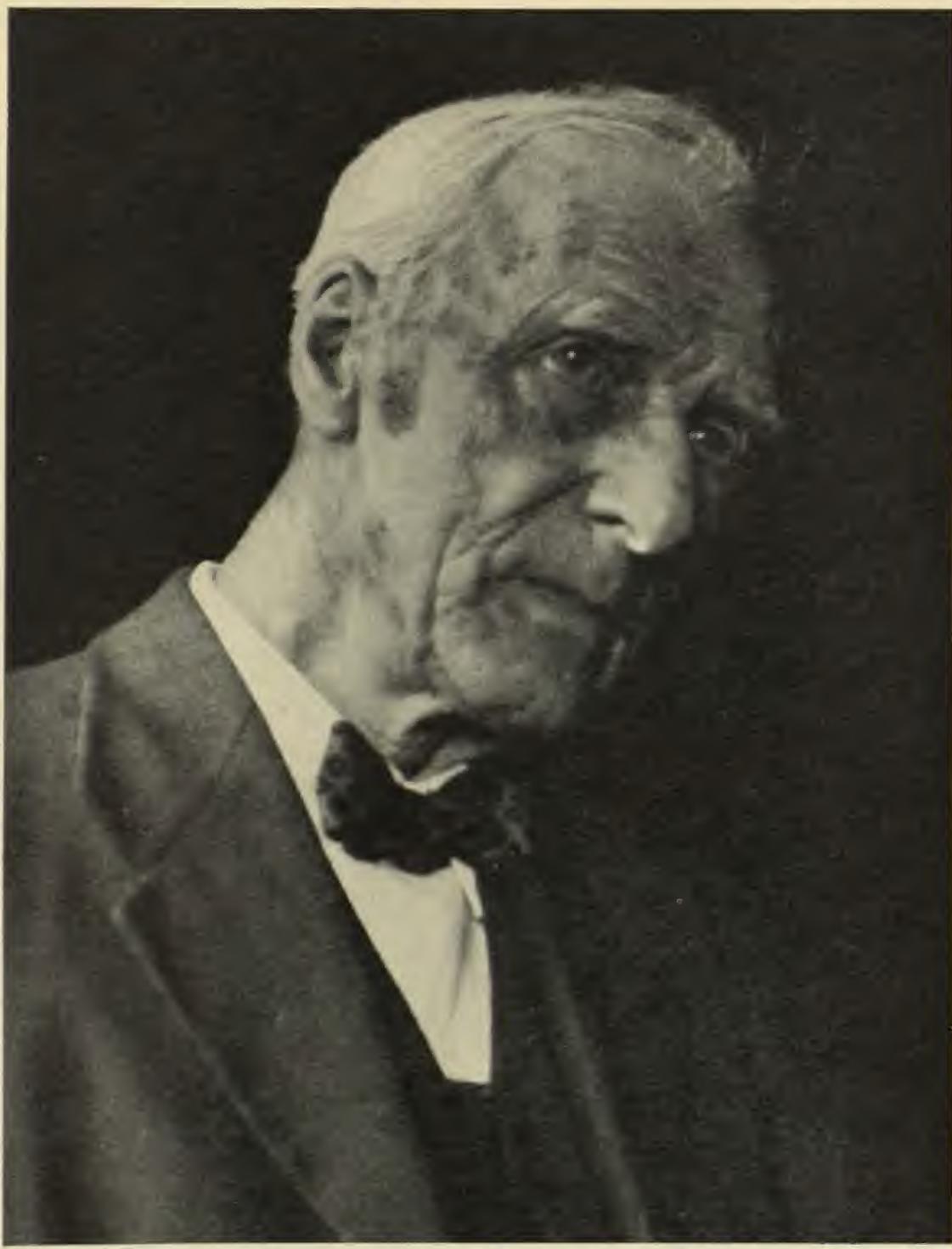
#### Notes

<sup>1</sup> 'Cargo Cult Activity in Tangu,' *Oceania*, Vol. XXIV, No. 4 (June, 1954), p. 246.

<sup>2</sup> *Loc. cit.*

<sup>3</sup> Sometimes 'collective punishments' are imposed—usually taking the form of forced labour over a period—and/or the ring-leaders may be imprisoned. At other times, a severe exhortation is considered sufficient. In any case, close surveillance thereafter is usual.

## OBITUARY



**I31** Sir Arthur Keith, F.R.S.: 1866-1955. With a portrait (by John Miller, A.R.P.S., reproduced by courtesy of C. A. Waite and Co., Ltd., publishers of Sir Arthur Keith's Autobiography, 1950)

Sir Arthur Keith, who died on 7 January, 1955, was President of the Royal Anthropological Institute for most of the war years 1914-1918, and I owe him much for kindness and help in connexion with a first paper to the Institute, a happy

memory which brought immediate compliance with the Hon. Editor's request for an obituary notice.

Arthur, sixth of ten children of John Keith and Jessie Macpherson, was born at Quarry Farm just north of Aberdeen on 5 February, 1866. John was a keen liberal, congregationalist and, later, a member of the Free Kirk deeply opposed to class privilege. His able and affectionate wife and he prospered and moved to Kinnermir, near the little town of Turriff. Arthur seemed destined

to be a farmer, but, in spite of inability to acquire the classical languages, he managed to pass the entrance examination for the medical faculty of Aberdeen University. Later at Aberdeen a short assistantship in a country practice led him to meet a young visitor from England who, years afterwards, became his wife. The granite city in those days was exporting men to high service, the scholar Sir Herbert Grierson, the lovable Lord Meston, Sir Patrick Duncan and Sir Harvey Adamson (Governors of South Africa and of Burma), Sir David Prain (Director of Kew), Sir Leslie Mackenzie (health administrator applying social anthropology in studying Scottish communities) and several others. Keith and Mackenzie were classmates.

Years of struggle were diversified by adventures as a medical officer in Siam during which studies of primate anatomy, especially of the gibbon, were a relief from many difficulties. His health, never robust, suffered but triumphed over tuberculosis and other troubles and he lived to be 88. His wife was wont to say that she hoped to be able to take care of him to the end, but she died 20 years before him after they had been married 36 years.

Teaching at the London Hospital led on to the Conservatorship of the Museum of the Royal College of Surgeons, which he held from 1908 to 1933. His lectures attracted public as well as student interest and led him to write articles on anthropological subjects, by-products of a keen, rather introspective mind, humanist and scientific as well as in a non-executive way artistic. He always showed, as well as held, that feelings, emotion perhaps, were strong elements in personality. Quick impressions might at times need later adjustment, but they were apt to be stimulating.

An enthusiastic Darwinian from student days, very critical of orthodoxies, Keith was early led to plead that the apes were our cousins rather than our ancestors, and he felt that *Homo sapiens* was of great antiquity, though he supported this view by accepting weak evidence for the great age of the Galley Hill skull. One should remember that this was long before Oakley's use of the

fluorine test. Marston's later find and study of the Swanscombe skull, and Oakley's test of its great age, put the thesis of high antiquity of *H. sapiens* on a surer foundation. So much has been said about Piltdown that one need but say regretfully how mistaken Elliot Smith, Smith Woodward and Keith all were in associating skull and jaw. Keith was less confident than the others about reconstruction of the skull. He courageously put together, publicly, pieces of a skull that had been intentionally cut by anatomists, making a fair success that did not satisfy him.

The most important of his studies of ancient skulls was done after retirement, with Dr. T. D. McCown, on skulls found by Professor Dorothy Garrod, in Palestine. When more finds have been made, they may well shed more light on the relative positions of *H. sapiens* and his cousin *H. neandertalensis*.

Of Keith's many contributions to anatomical science one need not speak here; he more than maintained the scientific repute of his museum and made it in a large sense a centre of public interest as well as of research.

Keith's temperamental emphasis on feeling led him to think that warfare had played a great part, and a by no means wholly evil one, in the social evolution of mankind. This aspect of his thought was strengthened by long friendship with the able and controversial writer, Morley Roberts. Nevertheless, Keith's fervid patriotism did not interfere with his appreciation of German scientific work.

Keith's association with Sir Buckston Browne preserved Down House as a Darwin memorial, a tribute to Keith's reverence as well as to Browne's generosity.

Interest in our Institute—to which he delivered the Huxley Memorial Lecture in 1928—was a feature of Keith's life for many years, and he maintained it, at a distance, even after a breakdown in health made him leave London in 1933. His bequest of £500 to our funds and of a large number of separata to our Library is the last of the many efforts which he made for us. H. J. FLEURE

## SHORTER NOTE

**Notes on the Eton of the Southern French Cameroons.** By Philip Dark, M.A., Ph.D., Department of Anthropology, University College, London. With a text figure

**132** The only published information on the Eton consists of a few sentences in a monograph by Dugast,<sup>1</sup> who refers to the following unpublished sources<sup>2</sup>: two papers on their physical anthropology, a demographic report by Dr. Aujoulat and a film, made in 1935 and now residing in the Musée de l'Homme, on the Sso rite.

In October, 1954, during a tour in the French Cameroons,<sup>3</sup> I had occasion to visit the Eton village of Evodouala (see fig. 1),<sup>4</sup> some 80 kilometres from Yaoundé by road and track in a roughly north-westward direction. Evodouala is a short distance to the south-west of the right bank of the Ngobo river shortly before this joins the Sanaga river. Its position is not marked on any map known to me; it does not appear on Dugast's maps<sup>5</sup> of the Eton area.

Officially,<sup>6</sup> the Eton are considered as Bantu. It is not certain whether they should be included among the Beti or the Fang or Pahouin.<sup>7</sup> According to my informant, Chef Abbéjean,<sup>8</sup> the Eton consist of two divisions: the Eton of the East and the Eton of the West. This division is a European one but I was told that it existed before the advent of Europeans. The legend of the arrival of the Eton in their present area tells how a slave of the Beti, who was sent to look for salt by his chief, met at a rivet (Sanaga?) a slave of the Basa, who had been sent by his chief to look for meat. The Basa slave had salt and exchanged this for meat from the

Beti slave. The latter returned with salt to his master. This prompted the Beti to move into the area in search of salt and they were halted in their present area<sup>9</sup> by the Basa.



FIG. 1. EVODOUALA VILLAGE, FRENCH CAMEROONS

Each division of the Eton, the eastern and the western, is divided into three *tribus* (or, in English anthropological usage, clans). Of the three *tribus* of the western division my informant claimed to be of the Vog-onamic<sup>10</sup> *tribu*. This *tribu* was divided into four *clans* (in English, lineages), though this appears to be a some-

what artificial division. The *clan* consists of a number of extended families and each of these consists of a number of households. Besides these groups, the Eton recognize three categories: the Eton-Beti, who are chiefs and their close cognates, the Eton-Beloua, who are commoners, and the Beloua-Eton, who are slaves. A man's status in the society is determined by *clan* membership and his position in the rank hierarchy. Thus my informant, Chef Abbéjean, claimed to belong to the Eloum-boin *clan* of the Vog-onamié *tribu*; this *clan*, he said, was the chief's *clan* of the Vog-onamié *tribu*. He also calls himself a Beti as he is of the Eton-Beti grouping.

*Clan* exogamy is the rule though in earlier times marriage could only take place outside the *tribu*. Patrilineal descent is observed and residence is matrilocal up to the age of 10 years, after which it is patrilocal. A village consists of a number of hamlets and a *clan* may occupy one or more villages. An extended family consists of a number of households, the heads of which are related to the senior man by agnatic and affinal ties (since residence is matrilocal for the first few years of marriage), and occupies a hamlet. A household consists of a man and his wives and the children of his wives. Chef Abbéjean had four wives and 11 children by them. Polygamy is, however, rare and depends on economic circumstances.

The physical demarcation of a village is not apparent. The hamlet is the most noticeably distinct residential unit, consisting of a number of houses grouped fairly close together and separated from a similar unit by a distinct interval. Houses are of wood and bamboo used as vertical and horizontal elements, lashed together, and with mud filling the spaces between (see fig. 1).<sup>10</sup> Roofs are of thatch, though occasionally a tin roof is to be seen, as on

Abbéjean's house, and this is indicative of economic status. His house was larger than the typical house of the Eton.

Gardens are close to the houses and are enclosed. The following crops are grown: groundnuts, maize, *patates*, yams (3 kinds: *k'ssol*, *guinguila*, *guignam*), bananas, plantains, cassava, macabon.

Clothes worn are European. Only rarely is traditional African costume to be seen and this usually is worn by very old men.

#### Notes

<sup>1</sup> I. Dugast, 'Inventaire Ethnique du Sud-Cameroun,' *Mémoires I.F.A.N. (Centre du Cameroun)*, Série: Populations, No. 1, 1949, pp. 68-70.

<sup>2</sup> *Ibid.*, p. 70.

<sup>3</sup> This tour was made on behalf of the West African Institute of Social and Economic Research, Ibadan.

<sup>4</sup> This visit was made in company with M. Roland Diziain of I.R.C.A.M., Yaoundé, to whom I am most grateful for the opportunity of seeing something of the Eton area. Any errors in the information given cannot be ascribed to M. Diziain.

<sup>5</sup> *Ibid.*, p. 69.

<sup>6</sup> *Rapport Annuel du Gouvernement Français à l'Assemblée Générale des Nations Unies sur l'Administration du Cameroun placé sous la Tutelle de la France, Année 1952*, Paris, p. 14.

<sup>7</sup> I. Dugast, *ibid.*, p. 69.

<sup>8</sup> My informant was M. Abbéjean, Chef de la Subdivision de Evodouala and also Chef du Groupeinent.

<sup>9</sup> *Ibid.*, p. 69.

<sup>10</sup> A description, with excellent illustrations, of the construction of this type of house, typical of the southern French Cameroons, is to be found in the very fine publication *L'Habitat au Cameroun: Présentation des principaux types d'Habitat; Essai d'Adaptation aux Problèmes Actuels*, Publ. de l'Office de la Recherche Scientifique Outremer (Éditions de l'Union Française), Paris, 1952.

## REVIEWS

### GENERAL

**Studies in Ancient Technology, Vols. II & III.** By R. J. Forbes. Leiden (Brill), 1955. Pp. vi, 215; vi, 268. Price 16.50 guilders; 19 guilders

In these two volumes upon ancient technology, Professor Forbes covers a very wide field in time, techniques and civilizations. In Vol. II the history of irrigation and drainage in the Old World is considered at great length. In a chapter devoted to 'Power,' the various forms of water mill are discussed; the paddle wheel, the windmills of the East, and the evolution of the Western mill. A chapter covers 'Land Transport and Road Building'; here we find an interesting account of the evolution of roads and methods of construction, with emphasis upon the important traffic of the Greek and Roman worlds.

Although Professor Forbes says that the history of the devices used in ancient irrigation systems is still largely shrouded in mystery, he has managed to give us a very useful survey of such machines and methods as are known, and also, helpful theories concerning the development of the machine as applied to lifting water for irrigation purposes in Near Eastern antiquity. Of particular interest is the author's review of the evolution of water-lifting devices terminating with the water wheel, mill, and screw. If there is anything in the Greek tradition ascribing the Archimedean screw pump to Archytas of Tarentum, one wonders if the screw pump may be the prototype of the screw proper, rather than the reverse. In view of the undoubtedly high antiquity of the device, it is interesting to reflect that, in the case of at least one canal in this country, the Archimedean screw pump was employed to empty the canal locks up to the end of the last century. In the chapter on 'Power,' we have a survey of the various forms of water mill developed for the purpose of driving corn mills. The history of different types of water wheel, some vertically mounted as in the Norse mill, others with horizontal axles and undershot wheels, has been difficult to follow and the literature cannot be said to be well-

known to the majority of anthropologists. Professor Forbes's clear and valuable summary of the subject should remove any confusion, and will be of considerable help to archaeologists as well as to those engaged in the purely technological field of study. The section of Vol. I which is devoted to land transport and road-building covers and traffic in the Persian Empire, traffic in the Greek world, evolution of Roman roads, Roman materials and road-construction, and roads and traffic in the Middle Ages.

A complete survey of such a vast subject is, of course, impossible within the limits of some 60 pages; hence, the best part of the chapter is probably that devoted to what may be termed the engineering construction of, and the statistics of loads and traffics carried by various ancient roads. Fig. 32, showing the trade routes of prehistoric Europe, cannot be said to be up to the standard of illustration of the rest of the book. One hopes that a more adequate map will be given in future editions.

As the following main chapter headings of Vol. III indicate, Professor Forbes applies a technological approach to a number of interesting subjects: Cosmetics and Perfumes in Antiquity; Food; Alcoholic Beverages; Vinegar; Food in Classical Antiquity; Fermented Beverages—500 B.C. to A.D. 1500; Crushing and Grinding; Salts; Preservation Processes and Mummification; Paints; Pigments; Inks and Varnishes. All these subjects come within the field of anthropology or archaeology, but in these days of specialization, few anthropologists or archaeologists can be familiar with the technical details of such a varied list of processes. Hence, Professor Forbes's work in tracing the history, and giving a clear and scientific appreciation of the techniques, will be found most helpful. Within the limits of a review it is hardly possible to indicate the varied range in matters of technological interest dealt with in these volumes. To mention but one or two points, we may say that the chapter on the use of cosmetics, etc. is of particular interest in showing that in Egypt and Mesopotamia, there was considerable

knowledge during early periods concerning the properties of a number of minerals. For instance, in connexion with eye paints, the use of galena, malachite, stibnite and occasionally even orpiment and realgar, as well as the common ochres, seem to have been known. The use of these substances must at times have led to unforeseen results—it is of interest to note (Vol. III, p. 39) that white lead poisoning is not a modern complaint.

A complex, and not very well known subject, is the early use of paints and pigments. Apart from the basic pigments like the oxides of iron and manganese, many other substances were used; for instance, basic copper carbonate is mentioned as a foundation for one of the early blue pigments in ancient Egypt, and in classical times it appears that the use of yellow sulphide of arsenic was known. Here, the author's knowledge of the chemistry of materials enables him to give a clear and helpful summary of the history and methods of application of the various colouring agents. Again, in a different and mechanical field, the discussion of the evolution of methods of pressing, from the elementary bag press to the invention of the beam, screw, and wedge presses, show us that all the essential elements of the modern press were known to the Romans, and that it was not until the comparatively recent introduction of the hydraulic press that further theoretical and practical progress was made.

In conclusion, one must admire the great amount of highly useful and often original information which Professor Forbes has managed to compress into two medium-sized volumes, and compliment him upon furnishing such adequate references to each chapter. Certainly the volumes may be recommended to anthropologists and archaeologists seeking an up-to-date survey, and an excellent guide to Ancient Technology. H. H. COGHLAN

**Sculpture of Primitive Man.** By Warner Muensterberger with Wendy L. Muensterberger. London (Thames & Hudson), 1955. Pp. 46, 136 photogravure plates, 2 colour plates. Price £2 10s.

The bulk of this book consists of excellent large-scale reproductions of sculptures from so-called primitive peoples in Africa, Asia and North America. The reproductions show a collection of typical and often famous pieces of high æsthetic standards. The text gives a clear and reliable survey over what is known of the conditions of primitive sculpture. In short, the book can be recommended as a popular introduction to this field.

There is, however, one passage which might be discussed with some zeal, not because of its revolutionary novelty but because it presents an opinion which one will often meet in other connexions. The Muensterbergers claim that a correct appreciation of primitive art must be based on other standards than those by which western works are judged, and that it is necessary to know the place of the primitive art object in its society in order to judge it rightly. To this the occidental art critic would reply that knowledge certainly is of great value in dealing with objects belonging to different cultures in so far as one wants to estimate their importance in their natural milieu, etc., but that as soon as one applies the word 'art' this rule ceases to function. No amount of knowledge about an object—as far as an æsthetic point of view is involved—can have any influence upon the appreciation of its artistic quality. A single instance will immediately illustrate what the art critic means by this: Everybody knows all about yesterday's fashions, yet their style invariably seems dull and awkward. The cause of this seems to be that we have unconsciously changed spiritually, that we now respond to a new set of combinations of lines, colours, patterns and so on. We might also illustrate the same idea by the meaningless question: Will an object be more or less beautiful, when you learn that it has been used as a fan by a local chief's second wife?

It is consequently useless to tell the æsthetically minded person that he must learn more of the religious or social importance of objects. They will have to be judged by their formal and spiritual qualities as far as these are in themselves able directly to make an impression on the beholder.

It is true that this fact leaves us with no other criterion but our own judgment to decide what objects are to be appreciated as works of art. As we have seen, centuries passed before Negro art

could find a response in the minds of Occidental artists and amateurs. Other groups of objects which we do not appreciate as art today are likely to be appreciated in the future, when our own standards will have changed, so that new kinships are created.

It is regrettable that the art historian and the art critic have no timeless rules by which to judge art, and that we are unable to enjoy all possible kinds of art; but, in fact, this incapacity of ours not only impedes our appreciation of some of the exotic cultures, it also extends to our own. Some of the discoveries in the near past, such as that of the glory of El Greco, are proof of this. The works of that great painter had to wait about 300 years until they could again be contemplated with satisfaction by aesthetically minded Europeans.

Yet, all in all, the present time seems to be rather open to a multitude of artistic expressions, which is proved not least by this book in which one finds a diversity of things of beauty which cause a joy to our generation.

BO WENNBERG

**Das doppelte Geschlecht: Ethnologische Studien zur Bisexualität in Ritus und Mythos.** By Hermann Baumann. Berlin (Reimer), 1955. Pp. 420, 5 maps. Price DM 30.00; bound, DM 36.00

After the war, Professor Baumann considered that the study of the relations between the ancient Indian, Near Eastern and Mediterranean civilizations on the one hand and Africa on the other was one of the most important tasks for an Africanist. The book under review is a result of this research work. What Baumann has succeeded in recovering had been lost to us not for hundreds but for thousands of years. He is the first to admit that his material is incomplete (p. 11), that many facts do not fit into his theory and that the farther away he moves from the centres of religious bisexual conceptions the more cautious he must be in his conjectures.

Baumann has demonstrated beyond doubt that bisexual deities are a comparatively late development to be ascribed to the ancient megalithic and agricultural civilizations (p. 250); that the conception of 'bisexual souls' is older and that cultic sex change was and is aiming at a 'higher unity' and thus at a strengthening of Life's essence. His diffusion theory (map, p. 372), although put forward in very cautious wording (pp. 252, 368) is less convincing. One notes how many question marks he himself puts to the 'supposed relations between different territories with bisexual myths and cultic sex change.' On the other hand, one would like to add a connecting link between the Western Sudan and what Baumann calls the 'Rhodesian Culture.'

Several of the phenomena discussed are so universal and are the expression of so natural an aspect for primitive humanity that it appears reasonable to regard them as autochthonous. Baumann's distribution maps lend support to this view which, however, in no way excludes later influences from other culture groups. As an instance the cosmic egg may be mentioned. Baumann has written an extremely noteworthy section about it. He also repeatedly refers to it throughout his treatise. Yet its implication as an early substratum of bisexuality in rite and myth has not been sufficiently taken into account. If this had been done, one would find fewer assumptions of bisexuality, and more of the immense number of facts collected would have found their natural place. Etymology proves e.g. that Omorka was certainly the 'Egg.' This is presexual, but potentially bisexual, and that is exactly what at least several African creators of High Gods are, whose names are in fact connected with the egg (Kiumbi, Mwari, Mboli, etc.). Baumann, too, has noted this vagueness (pp. 206f.). To the bisexual deities of the Western Sudan (pp. 200ff.) one could add Koma (Delafosse, *Haut-Sénégal-Niger*, pp. 174, 177), whose 'tube en fer sur laquelle on souffle' connects him not only with the Bambara Pemba (p. 196) but also with ancient folklore traditions in Southern Rhodesia. The Ibo tale about eggs changing sex reminds us of the same belief among the Thonga (Junod, *Life*, Vol. I, p. 101). The primeval round human beings without arms and legs are most easily explained out of the Egg conception. The Bambara cosmology (pp. 196ff.) is saturated with expressions and ideas pertaining to the egg.

The Bornean Cosmic Tree from which the children fall (p. 131) and still more the Iranian Reivas plant find their surprising parallel

in a Rhodesian rock painting (*Ethnos*, Vol. IV, 1955). The *jus primogenitum* (pp. 79f.) was formerly practised by the Karanga (Southern Rhodesia) for a whole month. What is said on p. 68 recalls Bullock, *The Mashona*, p. 87. Language questions are as a rule outside the scope of the book (see however pp. 102 and 82f., which raises the question of the Rhodesian 'ancients': *ntoro-ntoro* and the relation of this word to *Ntoro*, pp. 90ff., and *atoro*, p. 99). Yet the occurrence of only one common noun class in all Bantu languages for man and wife, without differentiation of the sexes, is worth consideration. Roeder's cosmic figure from Ambon, reproduced on p. 138, leads one's thought to the four divining dice used in the 'Rhodesian Culture' area, and ritual change of sex is found in Southern Rhodesia (p. 354) not only among people possessed by *jukwa* spirits, but also by *shavi* (Bullock, p. 144). Still more important is that when young girls are dedicated to Mwari, their 'breasts no longer come forth, they just stay like man' (N.A.D.A., 1956, p. 82). But here it should also be noted that the Lemba 'never put up stones' (Stayt, *J. R. Anthropol. Inst.*, Vol. LXI, p. 235). With the Luba, the bride is received by a female relative of the bridegroom, clad in a man's dress (Verhulpen, p. 264).

These few additions, which could easily be multiplied, are made to show how thought-provoking Baumann's book is. One can only hope that it will be studied, not least by fieldworkers so as to encourage them to collect new material on this question of paramount importance.

HARALD VON SICARD

**Culture and Human Fertility.** By F. Lorimer and others. Paris (U.N.E.S.C.O.), 1954. Pp. 310. Price £1 5s.

**I36** This book is a study of the relation of cultural conditions to fertility in non-industrial and transitional societies, by which are meant tribal and peasant societies. There are five parts to the book. Part One is a statement of general hypotheses by Professor Lorimer. Parts Two to Five are detailed fertility studies: Meyer Fortes's 'Demographic Field Study in Ashanti'; 'Some aspects of the relation of social conditions to human fertility in the Gold Coast', by K. A. Busia; 'Report on Fertility Surveys in Buganda and Buhaya, 1952,' by Audrey I. Richards and Priscilla Reining; and Giorgis Mottara's 'Brazilian Birth Rate, its Economic and Social Factors.'

These studies have a varied scope and content and their detailed review is precluded here. Nevertheless it may be said that, quite apart from their positive value at this stage of investigation of the fertility values and reproductive capabilities of societies where birth and death registration is not a routine, they clearly illustrate the many difficulties inherent in either fieldwork of this sort, or interpretation of official censuses. In the latter case, exemplified by the Brazilian study, the difficulties are largely those of handling statistically the census material, often gathered with insufficient appreciation of the sociological conditions involved, and indeed often inadequate due to administrative shortcomings. Some of these difficulties are obviated in work based on a sociological analysis, as in the Ashanti and Baganda-Buhaya studies. More exactly, they are exchanged for others no less formidable. First among these is the choice and supervision of the work of enumerators. Second is the selection of an adequate sampling technique. Third is the establishment of suitable criteria for age-estimation. Fourth is the evaluation of the time depth of the quantitative material obtained and its relevance for any accompanying sociological analysis. Thereafter, sociological enquiry assumes greater importance, in disentangling the values implicit in oral communication from the raw material for quantification. It is also important in bringing an analysis of social change to bear on the period to which the quantitative material refers. Readers will find all these problems touched upon with different emphases in the detailed studies, and their implications for the validity of the findings shirked in none.

Although Professor Lorimer has drawn from these studies in the development of his argument, the latter does not depend solely or directly upon them. Conversely the authors of the detailed studies are not explicitly committed to Professor Lorimer's hypotheses, although it appears that two of the studies (Busia, and Richards and Reining) were undertaken at his instigation. The theoretical section is thus not an introduction to the fertility material, and little attempt

has been made by the contributors to fit their material into the broad sweep of Professor Lorimer's theory. This discontinuity is the major defect of a book which is adventurous and energetic in the theoretical formulations of its major contributor, conscientious and precise in the detailed material of his collaborators, and, overall, a stimulating work which is timely and relevant both to the sociology of tribal and peasant societies and to what must be some of the most intractable issues of demography.

Professor Lorimer considers briefly the major poles of general theory concerning fertility in non-industrial societies. First, there is the 'optimum' population theory in which primitive societies are assumed to adjust population to resources and technology, usually by cultural limitation of fecundity. Secondly, there is the view (which I may perhaps be pardoned for labelling the 'maximization' theory) that primitive societies are oriented necessarily to high fertility, in view of the prevailing high mortality to which they are exposed. Thirdly, there is the proposition that primitive societies preserve a 'delicate balance' between pressures towards bearing children and tendencies to avoid birth.

Professor Lorimer accepts the validity of each of these standpoints for certain sets of social and environmental conditions. But his aim is to indicate more precisely what types of social structure and environmental factors condition, or are consistent with, the emergence of fertility patterns suggested by previous generalizations.

His first task is to give an idea of what is meant by 'high' and 'low' fecundity, by a technical exploration of the potential capacity of human stocks for the production of living offspring. This is of course in some ways a false problem, since it is never possible empirically to consider a human population as a biological stock unaffected by cultural conditions of its own making which primarily may have little to do with fertility. Nevertheless, Professor Lorimer considers the quantitative data of populations empirically characterized by high fertility, and proceeds to a statement of the hypothetical fecundity of a model population. This at least is a yardstick, although a knotty one.

The major variable correlates of fertility in primitive society are seen to be kinship organization on the one hand and on the other the environment, by which is meant not only the habitat with its effects on levels of subsistence and its demands on economic and technological organization, but also the social relations arising from competition between populations for the possession and exploitation of the habitat. A consideration of this triad forms the burden of Professor Lorimer's argument.

Relying heavily upon Radcliffe-Brown's *Introduction to African Systems of Kinship and Marriage*, Professor Lorimer lists four major categories of kinship system for analysis in terms of fertility or fertility values: simple cognatic systems, patrilineal systems, matrilineal systems, and 'a residual category of intermediate and complex kinship systems that deviate significantly from any of the three theoretically simpler types.'

Professor Lorimer first pursues the significance of patrilineal and matrilineal systems with regard to the fertility values and the environmental circumstances of societies in which such systems are found. Basing his argument largely on African societies such as Ashanti, Yao, Tallensi and Zulu, and also on Pueblo societies of the American Southwest and the Kazaks of Central Asia, he notes that corporate unilineal descent groups are found in societies with strong emphasis on unilineal descent. The organization of such societies is frequently oriented towards military power, and competition with other societies for the control of natural resources. Both the internal organization and external relations of such societies provide for the motivation and support of high fertility. In these societies the 'maximization' theory would appear to be the most appropriate. This section is ingeniously put together, and there are reasons for deducing that in his formulation of the expansive nature of societies with unilineal descent groups and the relation with the habitat which this implies, the author has leaned heavily on Fortes's, and secondarily Forte's, generalizations on the structure of unilineal descent groups. It may be argued that territorial expansion in such societies in the era before Arab or European influence began to be felt may most profitably be correlated, so far as reconstruction can go, not with militarism but with general underpopulation. Professor Lorimer

does not consider carefully enough this aspect of social change in Africa; he does not evaluate the different expansive qualities, even tentatively, of pastoral Hamitic societies, Zulu, Ashanti and Tallensi. Instead, he switches the reader's attention to a Central Asiatic pastoral society where purely militaristic expansion was of longer duration. Even here, there is a problem of herd demography rather than human demography, the crucial implications of which elude him.

So far, the argument has been developed with the kinship system as the starting point. Subsequently the point of departure is shifted to environmental conditions, and the reader is asked to consider societies in marginal and isolated areas, in which there is pronounced limitation of resources, such as the Eskimo, Australian aborigines, Andamanese, Chukchee, Koryak, Bushmen and Hottentots. Professor Lorimer accepts Lowie's observation that such societies lack corporate unilineal descent groups. Unable to review empirical data on fertility in such societies, the author readily inclines to the view that for such societies the 'optimum' fertility theory is most acceptable. Already committed to a heavy reliance on the data from *African Systems of Kinship and Marriage*, the author is bound to include the Lozi as an example of a society based on 'simple cognatic relations.' Although, again, there are no fertility data available, he cites Gluckman's statement of the significance of the Zambezi mounds as evidence of the limitations of the habitat. This is a hasty interpretation; Professor Gluckman's other work can be used to show this area to be one of diversified primary and craft production, far from eligible for consideration as a 'marginal' area. This section on 'marginal societies' is not convincing. It is embarked upon with a facile twist of emphasis; the values of fertility are discussed in little detail; there is no quantitative fertility material available. Professor Lorimer wisely confesses to all these shortcomings, except the first.

For examples illustrating the 'delicate balance' theory, the author turns exclusively to the Pacific and examines Tikopia, Buin, Lesu, Arapesh, and Mundugumor, where small islands, or natural barriers, enforce isolation, limitation of resources, and 'less opportunity for the consolidation of large expansive societies.' Tikopia is described as a classic case of rational control of fertility in a primitive society. A consideration of the remaining four societies suggests to the author that 'some conflicting institutional factors or ambivalence in attitudes towards fertility appear to have been present in many Pacific Island cultures.'

In the foregoing analysis, the effect of drastic changes associated with European influence on primitive societies is largely though not exclusively ignored, and the general assumption is that the fertility values discussed are 'traditional.' In a chapter entitled 'Uncontrolled Trends in Fertility,' Professor Lorimer discusses such drastic influences and gives examples of their adverse effects on populations traditionally conforming with the 'maximization' theory. From the Pacific, his material on Yap suggests a downward followed by an upward trend in fertility in a society traditionally conforming to the 'optimum' theory. That from Samoa supplies evidence of a continuation of fertility trends into the period of European influence in a society which appears to conform to the 'delicate balance' theory. Examination of data from the Indian populations of North America leads to the conclusion that here too, European influence resulted in a downward trend followed by an upward movement in fertility, particularly in Spanish America. This section is bedevilled throughout by a comparison, which can hardly be avoided, of pre-European fertility values, with such material as exists on reproductive performance. For an ideal presentation of the problem, these categories are not comparable. A discussion of either the change in values, or in performance, would be more satisfactory, but in every case, either the fertility quantification or the ethnographical data are absent. A way into the problem is surely afforded by a consideration of the change in other variables on which stress has previously been laid, but a systematic treatment of this is clearly beyond the scope of the book.

Professor Lorimer next turns from tribal society to pre-industrial agrarian civilization, the peasant societies of Asia and Europe. Here his argument takes a genetic tack, and he contrasts the primordial corporate unilineal basis of Asiatic society with the cognatic orientation of European pre-industrial society. In the development of agrarian society these have resulted in emphases on the group family

(joint or extended) on the one hand, and the nuclear family on the other. Consistent with his previous theory, he concludes that in Europe the fertility of agrarian society was practically congruent with its resources, while in Asia, the resultant high fertility is incongruous with the environment, leading to chronic famine conditions which are hardly arrested today. Complementary to this analysis is a review of the values of fertility inherent in the systems of the great religions practised in agrarian societies.

Finally the author reviews briefly, with reference to Europe, America, Japan, Brazil and Puerto Rico, the varying nature of the 'demographic transition'—that shift from high fertility and high mortality to lower fertility and lower mortality which accompanies the extension of technological diversity into the industrial era.

Some of the shortcomings of Professor Lorimer's analysis have been brought out here. His emphasis on the three major variables is not uniform. He tends to assume too readily that traditional values on high fertility are automatically accompanied by high fertility itself. He examines societies characterized by dramatic social change and decrease in fertility, yet shirks the evaluation of social change in societies whose traditional values of high fertility appear currently to be successfully implemented. Social change itself is not broken down in terms of more than one of the three major variables.

Fortunately, these shortcomings are accompanied by modesty, tentative formulation, and a ready recognition that much of the source material is inadequate for present purposes. Professor Lorimer has fashioned a large net, boldly and ingeniously. In places the net breaks, in places much escapes its wide mesh. But this book is a challenge to sociologists of tribal and peasant societies, and to demographers concerned with the problems they pose, to strengthen and refine the net of theory by the production of more detailed source material on the interrelation of forms of social organization and ecology with human fertility. The study of each of these variables cannot fail to benefit from such approaches. The remaining studies in *Culture and Human Fertility* follow the few steps already made in this direction.

DERRICK J. STENNING

**Les Fresques de Hans Erni, ou la Part du Peintre en Ethnographie.** By Jean Gabus. Neuchâtel (*La Baconnière*), 1955. Pp. 40.

Divided into two main parts, this book deals first with the duties of a museum to its public, and secondly with the artist's approach to their fulfilment. The author's view that the museum is an instrument of education which should by the nature and arrangement of its exhibitions arouse and foster a balanced interest, whether the collections be ethnography, folk art or natural history, is borne out in the opening paragraphs which touch on events and personalities of the past few centuries and their influence upon museums and our present *modus vivendi*.

The method devised at Neuchâtel and its execution in the capable hands of Hans Erni, whose art graces the 40 pages of this book, is dealt with in the second part, and pays particular attention to one of his several murals. This, 89 square metres in size, depicts the adventures of man in his search for betterment, and logically but conjunctively covers a very wide range of science, ethnography and art, portraying personalities from Buddha to Einstein, rock paintings of Europe and Africa to the work of Leonardo da Vinci, and the mechanics of an early chariot to those of an atomic power station, connecting Asia to Europe to Africa and, finally, to America.

Pleasantly produced and well illustrated, here is an interesting modern approach to exhibition technique which will provide food for thought for many museums and offer something which may well be of use to the organizers of national exhibitions and trade fairs.

H. J. GOWERS

**The Story of Our Ancestors.** By May Edel. London (Bodley Head), 1956. Pp. 199. Price 12s. 6d. **Man in Search of His Ancestors.** By André Senet. London (Allen & Unwin), 1955. Pp. xii, 274. Price £1.1s.

These books make no serious contribution to our knowledge of man and his ancestors, but as popular accounts or bedtime reading they have a certain value.

May Edel's book, written primarily for children, presents the

story of human evolution in an original and attractive way. It explains in simple terms how the Primates became progressively independent of their environment as they evolved; and the most unscientific of readers may readily understand how the fossil record of early apes and men is interpreted and assessed by the experts. The development of man's bodily organization is traced from simplest origins, and his emergence as a social animal, as toolmaker and artist, is fixed, so to speak, in perspective. But for some gross errors in syntax, annoying witticisms and general lack of style, this volume might have been recommended for inclusion on any bookshelf as a pleasant introduction to the study of man. Herbert Danska's illustrations are ingenious and charming.

**Chinese Spirit-Medium Cults in Singapore.** By Alan J. A. Elliott. London (Dept. of Anthropology, London School of Economics), 1955. Pp. 179. Price 18s.

**I39**

Chinese religion in Malaya consists of a bewildering variety of practices and beliefs, guilds and organizations. In different times and localities in China religious belief has found expression in different ways, and the immigrant to Malaya has brought with him those forms popular in his home village, district or province at the time of his departure. These have decayed or flourished according to the particular circumstances of Malaya's social and political development. Any comprehensive study of contemporary overseas religion is therefore a formidable task and Mr. Elliott is to be congratulated for this first full-length account of one of the major forms of religious expression in Singapore today. Although the author says that he intends this account more for those generally or professionally interested in overseas Chinese, it can hardly fail to come within the sphere of interest of both sociologist and student of Chinese religion.

Within the religious framework, Chinese make a distinction between named systems of beliefs and practices, which can be 'entered' (*Ju Tao* or *Chiao*), and that general, anonymous body of beliefs and practices which is highly syncretic, but lacking in synthesis. The ostensible purpose of those entering a system, be it Buddhist, Taoist or one of the syncretic *Tao* claiming to be 'three religions in one,' is spiritual progress. The underlying motive of anonymous religion, which has been called variously 'folk,' 'peasant' and 'mass' religion, is the obtaining of material benefit through propitiation of spiritual beings. Mr. Elliott calls this religion *Shenism* since the *shen* are a class of spiritual beings largely concerned with the fortunes of men. *Shenism* forms the basis of medium-cult beliefs and in this account Buddhas and Bodhisattvas are referred to as *shen* in their role as dispensers of material aid.

A major function of the cults is to provide for spiritual aid on personal and medical problems *via* a medium. Chapter VIII contains an interesting analysis of some of the more frequent cases brought before mediums for advice. *Shen* are not beyond giving moral advice through mediums too, and several such moral statements are given in an appendix. They are reminiscent of the poems found in 'good books' and pamphlets sold in many Singapore temples and which urge the cultivation of a spiritual life. An additional attraction of cults is in the physical feats performed by mediums. Detailed accounts are given of these together with descriptions of nail beds, sword ladders and other paraphernalia of mortification used in these performances. Cults specializing in spectacular displays, however, are considered by Mr. Elliott to be 'less reputable.' 'Rather sordid displays' are involved, and a 'low grade' of mediumship perpetuated. More disreputable cults have been omitted from detailed reference (p. 9). How far this omission affects the general picture of cult activities and organization which this monograph gives it is difficult to assess.

The organization of cults depends on no religious prototype. There is 'no established system by which promoters and ritual assistants work together with the *dang-ki* (medium), or by which they allocate duties among themselves.' The only instances of a relatively clear pattern of authority and rights, according to the author, is when the promoters are a kin group or a secret society. Similarities between cults are due more to their similar basis in common homeland tradition.

The second book is a somewhat more scholarly account of paleontology and prehistory. The translation from the original French is, on the whole, competent. With a historical approach, the author examines the human fossil record in some detail, and treats prehistoric life and art before turning to a general account of primate evolution. Since the Primates are mere newcomers in the geological record, Senet next examines the evolution of earlier forms of life—primitive mammals, reptiles, fishes, invertebrates. For the anthropologist, the value of this and other sections of the book is doubtful, but for the general reader there are many interesting topics, accurately discussed and fully illustrated.

M. PATRICIA WOOD

## ASIA

There is also no consistent form of inter-cult organization; the basis of inter-cult relationships is in the rendering of reciprocal services. It is not absolutely clear, however, how reciprocal relationships develop between some cults and not others. Widespread affiliations exist mainly among the less reputable cults. One clique of promoters is mentioned which has friendly relations with 20-30 mediums and influences about a quarter of all cults. We are not told how this quite considerable influence was obtained and is maintained. There is a hint that the master-pupil relationship operates in medium-training as in most branches of Chinese esoteric religion. 'The promotion of spirit mediumship is . . . an occult craft . . . it is reputed that masters are often unwilling to reveal the whole extent of their knowledge to their pupils.' If this relationship exists it is surprising that it has not as in other spheres of Chinese religion in Singapore, given rise to a religious 'family' system and had, as elsewhere, at least some effect on the organizational pattern. 'Family' connexions in Singapore have often considerable influence in matters of professional allegiances and rivalries, and form the basis of much inter-organizational co-operation.

Mr. Elliott is particularly informative on the economics of medium cults, the balance of relationship between promoters and medium and the reasons for the rise and decay of cults. On matters of training and qualifications for practice there are, however, one or two points that are not very clear. We are told that there is no priestly hierarchy, yet four stages of training are mentioned, each associated with certain occult attainments. Stage three is reached when a medium is able to dispense medical advice, but it is not clear how this proficiency is obtained and who are the recognizers of it. Stage four involves a study of the occult with a master and initiation into a brotherhood. This would seem to have organizational significance but apparently not many mediums reach this stage. These four stages are reminiscent of the five professional degrees of Cantonese Taoists of the *Cheng I* School in Singapore. They, however, train within a 'family' and later a band of priests associate professionally on a basis of 'family' relationship and a recognition of the stages of competence reached by each of its members.

Included in this monograph are accounts of specialized branches of *Shenism* including raising of souls with which women are mainly concerned. Also some of the more general aspects of overseas Chinese religion are discussed and there is an amusing account of the exorcizing of a spirit haunting the servants' quarters in a European house.

In view of the obvious overlap that his field of study has with secret society activities and organization, Mr. Elliott is to be particularly commended for the wealth of detail which he has obtained and which he presents in such a lively and sympathetic manner.

MARJORIE TOPLEY

**Water Into Wine: A Study of Ritual Idiom in the Middle East.** By E. S. Drower. London (Murray), 1956. Pp. 273, illustrations. Price 25s.

**I40** In the field of Mandean studies Lady Drower has an assured position as an accurate and well informed observer of current rites and customs familiar only in restricted circles outside the Near East. In the present volume she has extended the scope of her investigations to include the ritual meals of the Parseis, Jews, Yazidis and the Lesser Eastern Churches both Orthodox and

Uniate of the Byzantine Rite, together with those of the separated Monophysite Armenians, Jacobites, Copts and Abyssinians, and the Nestorian Assyrian schism. Having lived for many years in Iraq and Iran, in addition to her linguistic equipment, Lady Drower has succeeded in establishing valuable contacts with and gaining the confidence of those responsible for the performance of the rites she has recorded and described with sound judgment and accuracy.

The book opens with a brief survey of renewal, seasonal rites and observances connected with ritual food and drink, the sacred marriage, atonement and ransom, and communion with the living and the dead. A detailed account is given of magico-religious practices based on the symbolical nature of edible grains interpreted in terms of 'the bread of life,' and on wine as a 'life-fluid.' With the aid of excellent illustrations the elaborate preparation of the sacramental elements is described in detail, together with their significance and the various uses to which they are put in Jewish, Parsi, Mandean and Christian sacred meals.

The second section is devoted to an examination of the celebration of the Liturgy in the Russian Orthodox Church and in the Lesser Eastern Churches beginning with the Prothesis, usually conducted before the congregation assembles, and followed by the subsequent course of priestly actions culminating in the Consecration, the Fraction, the Commixture and the Communion, the various divergencies in the several rites being duly noted. The concluding part of the book deals with the Parsi ritual meals and liturgical rites viewed in relation to their Mandean counterparts, together with a brief survey of the fire-temple ritual, described in greater detail in her article in the *J. R. Anthropol. Inst.*, 1944. In the light of fuller knowledge some of her earlier accounts of Mandean rites are now revised and supplemented.

As a comparative study of the ritual idiom in the Middle East the book is of interest to and importance for the anthropologist both as a first-hand record of an aspect of the subject which is obscure and little known, and, as Professor Hooke says in his Foreword, for the material it supplies for the investigation of the structural content of the special type of 'ritual pattern' to which it conforms.

E. O. JAMES

**Die Reisefeste bei den Ngada.** By Hermann Bader. Mödling, near Vienna (St. Gabriel), 1953. Pp. viii, 146

**I41** This is a contribution of great value to our understanding of the meaning of puberty rites among primitive peoples. The author is a missionary of high intelligence who spent nine years from 1931 to 1940 among the Ngada, a tribe of some 40,000 inhabitants occupying a plateau in the interior of Flores in Indonesia, encouraged in his researches by R. P. Paul Arndt. The method of enquiry follows that of Jensen in his work *Hainuwele*, a classic on Ceram which is not well enough known in this country.

The book is a model of concise presentation, based mainly on the author's own fieldwork but also including many references to other sources. Its central point of interest is puberty rites among the Ngada themselves, but is not confined to them, comparison being made with similar rites among their neighbours on Flores itself and some of the nearer islands. The main rites described and analysed are: incision of boys (and also, as a derivative of it, the cutting of the clitoris in girls); the filing of the incisor teeth in girls (sometimes also copied in the case of boys) together with the blackening of teeth which is an important motif having to do with the darkness of the New Moon; the boring of the ears for ear-rings; and tattooing. These are all discussed in relation to a strong primitive matriarchal element, to an endogamous patrilineal clan-totemism considered to have been later imposed on it, and to a megalithic element of uncertain age but thought to have been introduced from north India.

The puberty rites are all concentrated on the problem of fertility, including that of crops but primarily in marriage, i.e. of raising children as an extension of personality into the future, and this depends in turn on the individual boy or girl being himself or herself an extension from the past in a cultural sense through faithfully following the precepts of the ancestral ghosts. Such ghosts represent both cultural and spiritual values in opposition to purely biological ones, and it is to maintain and further these specifically

human characteristics which separate mankind off from the animal kingdom that these rites are performed. These all consist in bodily mutilations that are therefore considered as being beautiful. One of the main celebrations is the *kush-bue* rite accompanying the filing and blackening of girls' teeth at about the time of the first menstruation in preparation for marriage. The author considers this to be the most basic of all puberty rites in this neighbourhood, more so even than incision for the boys, to which it corresponds. It is evident from what he says that the girl's teeth are, in their aggressive function, the female equivalent to the boy's male member, and therefore have to be symbolically sacrificed. This sacrifice is closely connected with the moon. The operation takes place during the three days of the new moon, i.e. the period when the moon itself is 'absent' or 'black.' Everything connected with this is 'black.' The girls during this time of their seclusion may not see the sun (the symbol of the boy's phallus), but only stroll abroad at night. What is left of their teeth is blackened, so that they may not be seen. In other words, aggression has to be transformed into acceptance, and this is 'beautiful,' as the shiny blackness of the remaining teeth is held to be the finest cosmetic. The rite is at one and the same time symbolic of the girl's acceptance of her feminine role and a protection against the boys' too pressing pre-marital demands, since during the time of all these puberty rites the consciousness of the sex motive is heightened in all sorts of ways but its consummation delayed. This makes sex 'human' as opposed to purely 'animal,' which is what girls' teeth are supposed to look like if unfiled, resembling those of dogs, pigs or buffaloes.

The book is full of highly suggestive material of this kind but written in a matter-of-fact style that inspires confidence in its objectivity. Each rite is first described and then discussed from different points of view. The incision of the boys is thus viewed separately from the biological, psychological, social and religious angles, and the filing and blackening of girls' teeth from that of their own fertility, together with the social and religious factors involved, and from the purely secular angle of aesthetic embellishment.

What is not stated, but can be deduced from all this material, is that by human 'fertility' is meant not actual children, but the fertilization of social and spiritual life resulting from the regulation of the sex instinct and the consequent transformation of part of it into values tending towards the development of character in individuals and therefore the better functioning of society.

It is a short book well worth reading. JOHN LAYARD

**The Tribes and Castes of West Bengal: Census of India, 1951.**  
Edited by A. Mitra. Alipore (West Bengal Govt. Press),

**I42** 1953. Pp. 414

The decision of the Indian government to ban the separate listing of castes in the 1951 and subsequent censuses will have been regretted by all sociologists. The volumes of past censuses dating back to 1872 are a unique and invaluable store of information such as no other civilization can provide. But sociologists will also be able to appreciate the wisdom of the change. Successive censuses have been used by castes in their struggle for status and dissident groups have taken the opportunity to establish themselves as castes in their own right. The printed word was invested with authority and the printed tables of castes became the source of bitter disputes. Whether, as some Indian officials would believe, caste was perpetuated in this manner is doubtful; it is more likely that the vitality of the system showed itself in the exploitation of the census as it does in the publication of caste periodicals and caste histories. However, when a government is in a position to modify this caste consciousness which makes for disunity, it is surely wise to do so.

The present volume, prepared and prefaced by A. Mitra, adds very little to our knowledge of the caste system in Bengal and reads more like a *Festschrift* to past scholarship. There are generous selections from Dalton, Sherring, Risley, Gait and O'Malley, most of which refer to nineteenth-century material. It is well worth having some of this collected together in one volume but some of it is merely interesting, some worthless; who for instance is interested in Bernard Shaw's views on *kulinism*?

There are three original contributions: Kumar Ray on 'The

Artisan Castes of West Bengal and their Craft' and Bhattacharya on 'Dharma Puja' provide material of ethnographic importance. Sengupta's article on 'The Racial Composition of the Bengalis' is described by Mitra as 'a piece of evaluation which will have both merit and scholarship to commend it to the notice of physical anthropologists.'

D. F. POCOCK

**Die Ornamentik der Jakuten.** By Ulla Johansen. *Wegweiser zur Völkerkunde, Part 3. Hamburg (Ethnographical Museum)*, 1954. Pp. 205, 13 plates

I43

The title of this work is parallel to that of *Ornamentik der Ob-Ugrier* by Tyyni Vahter, reviewed by me in MAN, 1956, 11. While the latter is a great illustrated description of a large material collection, the former is primarily a guide to the Yakut exhibits of the Hamburg Ethnographic Museum. As a consequence, it is typographically very unpretentious.

The Yakuts—about a quarter of a million in number—form a Soviet Republic in northern Siberia, a very large area on the river Lena. Their language is an independent branch of the Turkic family showing many phonetic peculiarities (*t>s*, *y>z*, *ð>w*, etc.), developed during the last 600 years, when the Yakuts have lived isolated from their relatives. This people has shown surprising vitality: neighbouring tribes as well as immigrants have largely become yakutized.

In her introduction the author draws the outlines of the ethnology and history of Yakuts. Various theories about their origin occupy perhaps too large a place: the results remain problematic and in any case have very little bearing on the main subject of the study. The techniques of the handicraft (pp. 29-53) as well as the social position of craftsmen (especially that of the blacksmiths) are briefly presented. The work of the blacksmith is coloured by deep magical concepts and is closely linked with shamanism.

The main part of the work is dedicated to a detailed study of the various ornamental motifs. A most peculiar feature of the Yakut ornament is the avoidance of all animal figures, used so abundantly by other Siberian peoples. The few animal motifs have been stylized to almost pure geometric figures, which are characteristic of Yakut ornament (cf. Vahter, pp. 29ff., 70ff.).

The above work by Vahter was published too late to be used by the author. Some useful older studies also seem to be missing among the quoted literature (e.g. Stasoff, *L'ornement russe*, articles of Chernetsov in *Sovetskaya Etnografiya*, and the great Yakut dictionary by Pekarskiy). Certain details might be criticized: e.g. the passage on p. 134, 'Neben dem blauen Himmel "Kök tengri" berichtet eine Inschrift von der "yayiz yir" der Hirsch-Erde,' where reference is also made to Mongol *dayir*, seems to be based on a misunderstanding: Uighur *yayiz* has here the meaning 'brown' corresponding to Middle Mongolian *dayir*, 'brown' in the *Secret History* §3 as the colour of a horse and §245 in the combination *dayir etugen*, 'the brown Earth' = Uighur *yayiz yir* (see e.g. Vladimirtsov, *Comptes Rendus de l'Académie des Sciences de l'U.R.S.S.*, 1929, p. 296).

As a guide and catalogue of the Yakut collections in Hamburg, the book of Dr. Johansen is of high quality. Without doubt it will also be of lasting scientific value.

PENTTI AALTO

**Songs in Lepanto-Igorot as it is Spoken at Bauko.** By Merle Vanoverbergh. C.I.C.M. Stud. Inst. Anthropol., Vol. VII. I44

Fribourg (Postvieux), Switzerland, 1952. Pp. 141

This collection of songs in Lepanto-Igorot is additional to those published in *Anthropos* 14/15, 1919-20 and in subsequent issues, up till 1949. Ten articles appeared in the journal, representing somewhat less than two-thirds of the total collection. The author gathered these songs and other folklore of the Lepanto-Igorot, or Kankanay, some forty years ago. He found in 1952, in Bauko, one

of the Kankanay towns in the Mountain Province of Luzon, Philippine Islands, that almost no one understood them any more, except a few old people. At present Christians and educated pagans form the majority of the Lepanto-Igorot of whom the total number is approximately 40,000.

The songs have been translated into English, the translation having been kept as close to the Igorot form as possible. Once the reader has been accustomed to the typical expressions in these Igorot songs—as for instance the interjection: 'they say'—the trend of thought is easy to follow. Many songs are delightful because of their poetical sense, as for example on p. 32: 'their child goes, then, they say, it encounters a burnt deer, then, they say, it thinks it is its mother, it looks at her eyes, and they are its mother's eyes.'

Dramatic vivacity is a common characteristic of *day-én* songs, the contents of the present work. These are sung universally, by women as well as men, sometimes by children, at work or play, in the house or in the field and at any time of the day. Other types of songs, *dain* songs used in sacrifices, swinging songs, mourning songs, wedding songs and fetch-wood-songs were presented in the aforementioned issues of *Anthropos*. The *day-én* songs have been divided into children's *day-én*, stories *day-én*, dialogues *day-én*, rock-the-cradle-songs and other *day-én*. As for the dialogues *day-én* many of them relate an argument between a woman and a man or *lalaki* (*lalaki* in Malay or Indonesian). The specific expression 'they say' is also to be found in the East Javanese way of telling a story: *djaréne* which is based on the Javanese word *udjar*, meaning 'to say.'

In *Day-én* 23 and *Day-én* 47 which both tell the story of a star caught and made a wife by a young man, some similarity is to be traced with a legend from East Java. In this legend a heavenly being left her wings near a pond and took a bath. A young man hid them under a rice-stack and forced the nymph to be his wife. She taught him how to pound the rice and after many months, after the birth of her child and when the rice supply had greatly diminished, she found her wings back and flew away. She left her husband and child behind, but also the method of preparing rice. Could it be that the Igorot word *talaw* also meant heavenly being or nymph?

The anthology would be greatly improved by a map of the region. An interesting vocabulary of terms borrowed from other languages and dialects has been included.

HURUSTIATI SUBANDRIO

**Indonesian Trade and Society.** By J. C. van Leur. *The Hague and Bandung (van Hoeve)*, 1955. Pp. 465. Price 12.50 florins

I45

This volume consists of English translations of the most important writings of J. C. van Leur (1908-42), an outstanding young Dutch historian who lost his life as a naval officer in the last war. It includes 'Early Asian Trade' (1934), 'Notes on the Study of Indonesian History' (1937), 'The World of Southeast Asia' (1939), and three reviews (1939-40). Van Leur's main themes are Hindu and Islamic influence on Indonesia, and the detailed study of trading operations throughout the archipelago. Probably the greatest general interest lies in his convincing refutation of the 'trader' and 'colonization' theories of the Hinduization of Indonesia, and in his outline of a theory of the cultural influence of Brahmins through the royal courts. It is unlikely, however, that the book will be of more specific interest or immediate value to social anthropologists.

The work is one of a series of 'Selected Studies on Indonesia by Dutch Scholars,' a highly commendable venture of translation and republication by the Royal Tropical Institute, Amsterdam, of important works which in Dutch might have continued to be little known to other European scholars. It is to be hoped that the series will flourish, and that among its subsequent publications there will be other works of this standard and of perhaps closer relevance to anthropological problems.

RODNEY NEEDHAM

## OCEANIA

**The Maori and His Religion—In its Non-Ritualistic Aspects (with a Danish Summary).** By J. Prytz Johansen. Copenhagen (Munksgaard), 1954. Pp. 298. Price 25 kroner

I46

The title of this book is misleading—even when one takes account of the qualification it contains—for a good deal of

what Dr. Johansen presents us with has very little to do with religion at all. Nor does he concern himself with an over-all theme. Rather, his book takes the form of ten loosely connected 'essays' on various ruling concepts and institutions of ancient Maori culture. Thus there are chapters on: The Kinship Group; Life and Honour; *Mana*; The

Treasure and the Gift; The Name; Fate and the Gift of Fortune; The Ancestors and History; *Tapu*; Woman; and Mind and Spirit. One is puzzled indeed why Dr. Johansen should have chosen the title he did, for early in his Preface he states plainly that the core of his book's subject-matter is: 'what might be termed the Maori's culturally determined personality as it was before European culture had interfered decisively.' This is a somewhat truer indication of its contents.

A translation into English of a doctoral dissertation of the University of Copenhagen, Dr. Johansen's book is scholarly and erudite and contains much of great interest for students of the Maori. Of the soundness and fruitfulness of its general approach, however, some doubts may be voiced. Throughout the whole of his book, Dr. Johansen leans heavily on a single method, and this method is a kind of linguistic analysis. So, in chapter after chapter we are regaled with very detailed discussions of a series of Maori terms and of their cultural connotations. For instance, the chapter on 'Life and Honour' begins: 'There is a word which by its applications can teach us a great deal of what life is to the Maori. It is the word *tupu*, to unfold one's nature.' Many pages of exemplification follow. And in the chapter called 'The Treasure and the Gift' we find this sentence: 'The whole relationship of the Maori to his possessions, in particular, the treasure, can be concentrated in one word, *manaaki*.' This then is Dr. Johansen's method.

Though semasiological studies be more fashionable in some places than others, all social anthropologists are, I think, agreed on their fundamental importance. But fine probing into the meanings of native words is, I would suggest, only really justified when it is supported by all those other observations of social behaviour which a fieldworker normally makes. Standing alone it tends to be evidence of a slender if not a precious kind. In short, while Dr. Johansen's discussions of the meanings of Maori terms are of interest in themselves, they fall far short of providing an adequate account of those aspects of ancient Maori culture to which they refer.

Had he wished, Dr. Johansen could have deployed all of the pertinent evidence that exists in the very extensive literature on the Maori. Professor Raymond Firth's *Primitive Economics of the New Zealand Maori* (also a Ph.D. thesis) is a brilliant example of what can be achieved from a comprehensive study of the sources. But Dr. Johansen, it would appear, follows his method of textual analysis because he believes it to have notable advantages. In the Appendix

to his book he argues: 'In the case of the Maoris there is no doubt that it is among the texts that we have to look for the sources of the very highest value. It is of the greatest importance that the texts allow us to become familiar with the notions in which a people has itself expressed their experiences, also such concepts as may be missed by the ethnographer in the field.' Texts, Dr. Johansen goes on to claim, 'are full of living samples of the life of the people, which release us from the rigid generalizations and cultural blinkers which one risks to find in travellers and ethnographers.'

There is in Dr. Johansen's claim a considerable measure of truth. Texts, however, like all other observations, may vary greatly in comprehensiveness, authenticity and reliability. There is, I would suggest, nothing sacrosanct in a text; it is as much a piece of ethnographic reporting as, say, a description of how a priest performs a rite. And in both cases the reporting may be good or bad. There is, indeed, a conspicuous illustration of this point in Dr. Johansen's own book. One of the sources of which Dr. Johansen makes leading use is John White's *The Ancient History of the Maori*. (There are about 450 references to this work in the 1,240 or so footnotes of Dr. Johansen's book.) But *The Ancient History of the Maori*, it so happens, has long been notorious for the unreliability of many of its texts. Herbert Williams, in the Introduction of June, 1917, to his Fifth Edition of *A Dictionary of the Maori Language* singles out John White's six-volume work for devastating critical comment and gives it as his judgment that it is impossible to accept White's unsupported authority 'for the form or meaning of a Maori word.' Since then further drastic judgments on the writings of 'the alembic White' have been made by George Graham, Johannes C. Andersen and others. White's texts, there can be little doubt, are rather more of 'a risk' than are the observations of the travellers and ethnographers Dr. Johansen so distrusts. (In fairness to Dr. Johansen it should be noted that he does, in his Appendix, devote several pages to discussing the reliability of White's texts; and he was, as he says, unaware of the criticisms that had been levelled against them until his book was 'almost finished'.)

Dr. Johansen's *The Maori and His Religion* contains much of interest and value, but it would have been a much better book if he had made a more catholic use of all the source materials available. Textual analysis is an important tool in the anthropologist's kit, but with this tool alone he cannot reach the integrated understanding that should be his goal.

J. D. FREEMAN

## CORRESPONDENCE

### Radiocarbon Dates

**I47** SIR.—May I through your columns point out what is, to my mind, a defect in the present method of presenting radiocarbon dates, and suggest a very simple remedy?

It appears to have become standard practice to present such dates in terms of years 'Before Present'—'Present' in this case being, of course, the year in which the determination of the age of a particular sample is actually made. To obtain the 'calendar' date—B.C. or A.D.—a simple subtraction sum is necessary, and to obtain the exact 'calendar' equivalent it is obviously essential that the year of the determination shall be known.

There seem sound reasons for the retention of a presentation in 'B.P.' dates (one most important being the fact that the 'Christian' calendrical convention is not current among very large sections of the world's population) and at the present time any possible error in calculating back to the 'calendar' date is so small as to be unimportant. But we may expect that archaeology will still be flourishing in 50 or 100 years' time, and further that some at any rate of the radiocarbon dates now accepted will then still be current. The error possible in calculating 'calendar' dates from these 'B.P.' dates, through mistake about the actual year in which the determination took place, would at that time be quite substantial and would of course increase continually with the passage of years.

This difficulty can be removed without any trouble, however,

by the simple expedient of adding, after the 'B.P.' date, the year of the determination, in brackets, thus: '4239±220 (1951)' (hypothetical example). No possible risk of mistake in the 'calendar' date could then arise. I commend this suggestion to the attention of all concerned.

Bristol

R. D. GREENAWAY

### Breath and Spirit. Cf. MAN, 1956, 48

**I48** SIR.—In the April issue is a statement by Dr. Audrey Butt that the association of breath and spirit '... can be traced back to early Hebrew'. No doubt it can, but what Dr. Audrey Butt appears unaware of is that before the Hebrews were ever heard of the Pyramids of Egypt were already ancient and that connected with the mummification of the Pharaoh was the ceremony of opening the mouth whereby the breath of life was restored to the deceased.

M. D. W. JEFFREYS  
University of the Witwatersrand, Johannesburg

### The Blackrock Cross. Cf. MAN, 1956, 102

**I49** The Hon. Editor much regrets that through an oversight it was not noted below the illustration of the Blackrock Cross in Mrs. Ettlinger's letter that the photograph was by Mr. H. S. Crawford and was published by permission of the National Museum of Ireland.



(a)



(b)



(c)

**STONE ALIGNMENTS IN RAICHUR DISTRICT, SOUTHERN HYDERABAD***(a) Krishna Bridge (No. 37); (b) Jamshed I (No. 31); (c) Jamshed II (No. 32)*

# THE STONE ALIGNMENTS OF SOUTHERN HYDERABAD\*

by

DR. F. R. ALLCHIN

*School of Oriental and African Studies, London*

**150** The first notice of the Indian 'megalithic' monuments which are discussed in this paper was in Colonel Meadows Taylor's account of the ancient remains in Surapur (1852). Taylor recorded four places at which lines or diagonal lines of stones were to be seen. Eighty years were to elapse before the Hyderabad Geological Survey, under the leadership of Captain Leonard Munn, published reports of many further alignments in its *Journal* (1934-41). A number of sites were also reported in the *Annual Report* of the Hyderabad Archaeological Department. From all these sources 24 are known. During my fieldwork in Hyderabad in 1952 a further 19 sites were discovered, of which this is the first report. The resulting distribution map (fig. 1) shows that so far alignments have only been found to the north of the Krishna



FIG. 1. DISTRIBUTION OF ALIGNMENTS IN SOUTHERN HYDERABAD

and Tungabhadra rivers in the Raichur, Gulbarga, Nalgonda, Mahbubnagar and Atraf-i-Balda districts of Hyderabad state. It is certain that many more sites await discovery in this area and it would be interesting to make further exploration in the adjoining districts to the south and east. Indeed until a search has been made there it is impossible to determine whether the absence of sites south of the rivers is a fact or merely the result of limited research.

The alignments consist of parallel lines of standing stones set out with mathematical precision. The lines are approximately oriented on the cardinal points in all recorded cases. The stones are spaced at carefully regulated intervals which may differ on the north-south and east-west axes. The intervals so far recorded vary between 15 and 40 feet. In one case an alignment is reported to have stones still standing of 14 to 16 feet in height; in another a single fallen stone has been noticed of 25 feet: but the most common heights for the stones are from three to six feet. The cross section of the longer stones is sub-rectangular, polygonal or near circular, whilst the shorter ones are often

barely worked boulders of conical form and irregular section. The diameter varies between two and four feet. The shorter stones are often mere boulders with little or no dressing; the longer ones are blocks which must have been quarried by means of fire-setting. The marks of chisels or drills, which are so often to be seen on mediæval or modern stonework, have never so far been reported. The rock chosen was usually local granite or gneiss or more rarely dolerite. In a few cases from Gulbarga district the local sandstone was used, as it was for stone-cist graves in the same area. In a very few instances the stones were carried to the site from a short distance, probably not more than a couple of miles (e.g. Nos. 8 and 12 of the Gazetteer). Although no accurate survey has yet been made of any site, it is possible to discern two varieties of arrangements, which we may term 'square' and 'diagonal.' In the first the stones are set in lines which form rectangles (fig. 2a).



FIG. 2. (a) DIAGRAMMATIC PLAN OF SQUARE ALIGNMENT, JAMSHED I (NO. 31), (b) IDEALIZED PLAN OF DIAGONAL ALIGNMENT

The smaller alignments are mainly of this sort and thus we find two with three rows of three stones, three with four rows of four, three with five rows of five, two with six rows of six and one with seven rows of seven stones. Some have other ratios as three rows of four, or four rows of five. In the second variety the stones of the even-numbered rows are offset half an interval, or rather are set in the centres of the squares formed by those of the odd numbered rows (fig. 2b). The effect is thus to stress the diagonal lines. The only examples which are known to belong to this variety are those with larger numbers of stones and it is probable that careful survey would reveal that these also had regular numbers of stones in the rows and that the even-numbered rows contained one less than the odd.

It has already been noticed that the rows are oriented roughly on the cardinal points. As far as can be determined this applies also to the diagonal alignments, although I have been unable to verify it. Of the ten examples which I was able to measure, no less than six lie between 15° and 20° east of north. I noticed a similar variation in the orientation of some of the stone-cist graves of Raichur, and Dr. Hunt

\* With Plate J, two text figures and a Gazetteer of Sites

recorded comparable figures for the graves east of Hyderabad (1). The meaning of this variation is not clear but it would seem to relate to the method adopted to establish the cardinal point. It may well be that the sunrise was employed as it is still in some Buddhist countries to orient religious structures, but the problem merits further study.

In considering the age and original purpose of the alignments it may be useful to notice their relationship to other sites. More than half the recorded examples are in the immediate vicinity of, or even adjoining, stone-cist or circle graves. The circle graves either may or may not have a cist buried in a pit. Several of the alignments lie on the outskirts of major settlement sites. Several are on the banks of streams. No less than eight sites have been recorded along about 50 miles of the modern road from Raichur to Mahbubnagar. The meaning of these observations is not clear as systematic search has mainly been limited to the vicinity of the roads, but it seems clear that the graves, alignments and settlements all stand in regular relationship and that the graves and alignments would appear to be generally contemporary. A clear picture of the relationship can be had at Piklihal, Maski or Jamshed and is briefly indicated in the Gazetteer of sites below. The hypothesis of the general contemporaneity of circle graves and alignments is reinforced by the excavation of a ten-foot square around one of the stones in Piklihal I. Immediately below the modern turf and humus the disintegrated granite subsoil was encountered. The stone itself had been erected in a pit of about 18 inches in depth. On the surface of the subsoil were several shallow depressions less than six inches deep and in one of these depressions was found a sherd of red-and-black ware of a type well known from the grave sites of the entire Peninsula. The hollows were perhaps connected with the mechanics of erecting the stone. The excavation revealed no further evidence, but seems to support the hypothesis that the alignments may be dated to roughly the same period as the graves. This would perhaps indicate that they may date from the last centuries B.C. or first centuries A.D., but when further evidence is available these limits may well be extended in either direction.

We have seen that the Hyderabad alignments are rows, or diagonal rows, of rough-hewn stones or boulders set out with mathematical precision and oriented to the cardinal points. With such features as criteria it is not easy to find comparable monuments in India. The nearest comparison to the plan is to be found in the *nava graha* (nine planets), carved in stone in human form, standing in three rows of three, and to be found in many South Indian temples. None of these, however, has any great antiquity. There is a solitary reference to a line of four standing stones in Cochin (2), but apparently it is a single line only. The Dimapur alignment, together with the other examples recorded by Hutton in Assam, is also clearly different (3). There are two types of stone, male and female, either of phallic form or forked. Stones of both types are carved in a manner which suggests the mediaeval temple stone mason. The characteristic plans of the Hyderabad specimens are not comparable with these twin lines of carved stones, and

it is difficult to assign any comparable date to the Assam monuments. Moreover, as Hutton has convincingly shown, there is a close connexion between them and the neighbouring Naga stone and wood memorial pillars, to which bulls were tethered for sacrifice.

What was the purpose for which these lines of great stones, often weighing several tons, were set up? Within my knowledge Sanskrit literature supplies no answer, nor does ancient Tamil. There are many references to stone memorial posts, warrior stones, and stones to which offerings are made, but never to alignments of such stones. Modern traditions in Hyderabad are not particularly helpful. The villagers refer to the stones in Telegu as *nilu rālu* or *emigu rālu*, standing stones or elephant stones, and in one case the story is told of villagers and their cattle petrified by curse. Taylor mentions the tradition that they mark the sites of kings' camping places and that horses and elephants were tethered to the stones. Dr. Mahadevan records an interesting custom associated with the monument at Hegragati. It is known as *mantraki* (Sanskrit: *mantra*, a charm, hence perhaps charm field or magician) and in times of epidemic the herds are driven around the stones to ward off disease. This recalls the 'cattle stones' of the Mysore country to which offerings are made in similar circumstances (4). Among the Koys of Godavari district it is reported that great stones are set up for commemorative purposes and that the tail of a cow or bullock which has been sacrificed is tied to the top (5). These customs seem to suggest commemorative and perhaps magical and prophylactic functions for the stones, and call to mind Hutton's conclusions. They also raise an interesting question of relationship of stone alignments and stone *yūpas* (sacrificial pillars) for in at least one case such pillars were set up in a group of four (6). The repeated proximity of graves and alignments might support the theory. There is, however, a second broad possibility which also deserves attention. The sites occur on the outskirts of settlements and on the lines of roads. Is it possible that they represent caravan halting places (or *dharma-sālā*) or that they were the sites of periodical markets or fairs? It is even possible that they combined these functions with those of commemoration and prophylaxis, but these are fields of speculation which at present are unpromising. The need is for further survey, particularly air survey, of the region, and for the accurate recording of sites. It is then for careful and complete excavation of a selected example. After this it may be possible to supply further information on the function of these unique and interesting monuments.

In conclusion I should like to acknowledge my gratitude to Professor C. Mahadevan of Andhra University for putting his valuable field notes at my disposal, and to Dr. P. Sreenivasarao, the Director of Archaeology, Hyderabad, and his staff for their co-operation in the excavations at Piklihal.

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3. J. H. Hutton, *J. R. Anthropol. Inst.*, Vol. LII (1922), 'Carved Monoliths at Dimapur and an Angami Naga Ceremony'; and *ibid.*, Vol. LIII (1923), 'Carved Monoliths at Jamuguri in Assam.'
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5. E. Thurston, *Ethnographic Notes*, Madras, 1907, pp. 155f.
6. A. S. Altekar, 'Three Maukhari Inscriptions on Yugas,' *Epi-graphia Indica*, Vol. XXIII, p. 42, etc.

*Gazetteer of Sites*

GULBARGA DISTRICT

1. *Vibhutihalli*. P. M. Meadows Taylor, *J. Bom. Br. R. Asiat. Soc.*, Vol. IV (1852), pp. 389f., and *P. R. Irish Acad.*, Vol. XXIV, pp. 352f.; S. K. Mukherjee, *J. Hyder. Geol. Surv.*, Vol. IV, Part 1, p. 54; *Annu. Rep. Hyder. Arch. Dept.*, 1940-41, p. 12 and Plate XIV b. The site is about 100 yards east of the Shahpur Road, one furlong north of the village of Vibhutihalli. Taylor describes it as of pink granite rocks three to four feet high, about 360 by 340 feet, with 22 rocks in a complete line. The orientation is north-south and there are stone-circle graves in the area. The air photograph in *Annu. Rep. Hyder. Arch. Dept.* shows it to be a diagonal alignment, with about 36 alternating rows of 15 or 14 stones. Known locally as the 'King's stables,' it is reputed to be a royal camping ground.

2. *Shahpur I*. M. Taylor, *loc. cit.*, p. 391. About one mile south of Shahpur, and two north of Vibhutihalli, near village of Rakhamgiri. Taylor describes the site as smaller than the former and near some circle graves.

3. *Shahpur II*. M. Taylor, *loc. cit.*, p. 392, and *loc. cit.*, p. 353; *Annu. Rep. Hyder. Arch. Dept.*, 1940-41, p. 12 and Plate XIVa. Close to the east gate of the town of Shahpur. Many stones have been removed (Taylor). One stone is 25 feet long.

4. *Ijeri*. M. Taylor, *P. R. Irish Acad.*, Vol. XXIV, p. 355. Near Ijeri village about 18 miles north of Shahpur. There are six rows of six stones with due north-south orientation (Taylor). Two cairns are situated among the stones. It is a square alignment.

5. *Nilogi*. Recorded on a map made by Munn and now in the office of the Geological Survey of Hyderabad. No other information.

6. *Hanamisagar*. C. Mahadevan, *J. Hyder. Geol. Surv.*, Vol. IV, Part 1, p. 157; Mahadevan, unpublished notes, pp. 8f.; *Annu. Rep. Hyder. Arch. Dept.*, 1940-41, p. 13. The site is one mile north-east of Hanamisagar village in Lambardi Thanda. The alignment is about 800 yards square, with about 1000 stones of porphyritic gneiss and trap-dyke rock. The average height is four to six feet, with a central enclosure about 100 feet square. Circle graves in the vicinity.

7. *Halbhari*. Mahadevan, *loc. cit.*, p. 157 and pp. 6f. West of the village near the Hanuman temple, about 10 miles south-west of Surapur. Consists of massive menhirs of gneiss; circle graves in the vicinity.

8. *Bachimatti*. Mahadevan, *loc. cit.*, p. 157 and p. 7. On an oval hill (1397') north-east of Bachimatti village. On a pegmatite hill; the stones are of gneiss and must be imported. The site is known locally as Gudachumaddi. There are circle graves nearby.

9. *Paramanand Hill*, *Halannardi*. Mahadevan, *loc. cit.*, p. 157 and p. 7. About one mile south of Benkanhalli. The site is on a quartz hillock known as Halannardi. There are three or four rows of gneiss boulders. Circle graves surround the monument.

10. *Chickanhall-Mangihal track*. Mahadevan, *loc. cit.* The site is a short distance north of No. 8 above. There are a number of much disturbed gneissic boulders.

11. *Mallur-Vitragal track*. Mahadevan, *loc. cit.* The site is to the east of Mallur village on the track to Vitragal and Benhatti. There are many graves in the neighbourhood.

12. *Rajan Kollur*. Mahadevan, *loc. cit.* Due east of the village on a mound of syenite. The stones are of shaly sandstone and imported. The site is near the junction of the Bhima sandstones with the gneissic rocks.

13. *Hegatgi*. Mahadevan, *loc. cit.* South-east of the village. The monument is locally known as *mantraki* and cattle are led round the stones, in times of epidemic, as a prophylactic. Only four stones remain erect.

MAHBUBNAGAR DISTRICT

14. *Mudumala*. L. S. Krishnamurthy, *J. Hyder. Geol. Surv.*, Vol. IV, Part 1, p. 86. The site is east of the village. No details are given.

15. *Murardoddi*. Krishnamurthy, *loc. cit.* The site is one mile south-west of Murardoddi village, and is near the Krishna River. It is known locally as *Nilu Rālu* (standing stones) and associated with petrified villagers and cattle. The alignment consists of six rows of six stones of 14 to 16 feet in height. It is about 200 feet square. There is a photograph of this impressive site in *J. Hyder. Geol. Surv.*

16. *Madhwaram*. Krishnamurthy, *loc. cit.* Just north of Madhwaram village and about two miles from Marikal. There are seven rows of seven stones about 150 feet square. Nearby are a single 'long pillar' and circle graves. The monument is known locally as *Nilu Rālu Celuka*.

17. *Kundapur-Sanganupalli track*. Krishnamurthy, *loc. cit.* South-east of hill 2132'. A number of fallen 'rough-hewn' stones.

18. *Kotakunda-Koilkunda track*. Krishnamurthy, *loc. cit.* On the bank of the Pedda Vagu river. There are about 22 stones, many now fallen. The site is known locally as *enigu rālu* (elephant stones).

19. *Hindupur-Gudabelur track*. Krishnamurthy, *loc. cit.* On a quartz ridge about half a mile west of the main road from Raichur-Mahbubnagar, and one mile north of the Krishna river bridge.

20. *Gudabelur*. Not previously reported. About 400 yards north-west of the temple and two miles north-east of the Krishna bridge. There are four rows of four stones of pink gneiss at intervals of about 36 feet. The orientation is 10° east of north. The stones are about four feet high.

21. *Gopalpur*. Not previously reported. Near the 75·7 mile stone on the Raichur-Mahbubnagar road, on the bank of the Pedda Vagu river about one mile west of Gopalpur village. There are four rows of three stones of pink gneiss. The orientation is approximately north-south.

22. *Devakadra*. Not previously reported. About two miles north of Devakadra on the Raichur-Mahbubnagar road at the 71·4 mile stone. There are about two dozen stones of grey gneiss, of four to six feet high.

23. *Mahbubnagar-Hyderabad Road I*. Not previously reported. The site is to the east of the road, at the 53·4 mile stone, about five miles north-east of Mahbubnagar. There are five rows of five stones about 150 feet square. The stones are of grey gneiss. There are circle graves adjacent to the site, and all are on the banks of a stream.

24. *Mahbubnagar-Hyderabad Road II*. Not previously reported. The site is to the east of the road at the 54·1 mile stone, within a mile of the previous site. The number of stones was not recorded.

25. *Mahbubnagar-Hyderabad Road III*. Not previously reported. East of the road at the 45·3 mile stone. There are four rows of four stones of grey gneiss. The orientation is roughly north-south. There are about 13 circle graves beside the alignment.

26. *Mahbubnagar-Hyderabad Road IV*. *Annu. Rep. Hyder. Arch. Dept.*, 1935-36, p. 10. Near the 47·5 mile stone. No further information.

RAICHUR DISTRICT

27. *Hardigudda*. L. J. Munn, *Hyder. Geol. Surv.*, Vol. III, Part 1, p. 82. About a mile and a half south-south-east of the village and two miles north of Manvi. No further details.

28. *Maski I*. Not previously described. West of Maski hill. There are three rows of three stones oriented 20° east of north.

29. *Maski II*. Not previously described. Several hundred yards south of No. 28. The number of stones is not clear.

30. *Maski III*. Not previously described. To the south of No. 29. There is a larger number of stones than in the previous examples but the plan is not now clear. The alignment adjoins stone terraces and wall foundations of doubtful age.

31. *Janshed I*. Not previously reported. One mile north-east of the village of Janshed and about two and a half south-east of Gadwal. This and the next five sites lie on both sides of a stream and seem to be related to a complex series of graves, building foundations and a small settlement site, Potla Pahad, where there are traces of a ruined fort. A ground stone axe was discovered in the rubble of the fort wall but no other evidence of any settlement earlier than the

early historic period. The alignment consists of four rows of four stones, four to six feet high and of grey gneiss. The intervals are 36 feet on the east-west axis and 28 feet on the north-south. The orientation is 20° east of north. It lies between two groups of circle graves. (See Plate Jb.)

32. *Jamshed II.* Not previously reported. The site is about 200 yards south-west of No. 31 and is cut by a modern track. There are five rows of five stones with an overall size of 120 by 102 feet. The rock is grey gneiss. The orientation is 15° east of north. (See Plate Jc.)

33. *Jamshed III.* Not previously reported. About 150 yards east of No. 32. There are about 20 stones of conical form, but few of more than three feet remain standing. The orientation is about 15° east of north.

34. *Jamshed IV.* Not previously reported. The site adjoins the Potla Pahad settlement area. There are four rows of five stones with intervals of 21 feet on the north-south and 22 feet on the east-west axes. The orientation is 15° east of north. At least two of the stones have been incorporated into the wall of the fort.

35. *Jamshed V.* Not previously reported. This site consists of three loosely scattered groups of aligned stones, perhaps three separate alignments in origin. One group includes seven rows of five stones with orientation of 20° east of north and intervals of 27 feet and 36 feet. There are more stones about 100 yards away and beyond them a further group of 17.

36. *Jamshed VI.* Not previously reported. The site lies on the bed of the dry stream midway between Nos. 34 and 35. There are three rows of three stones oriented 15° east of north.

37. *Krishna Bridge.* Not previously reported. To the west of the road from Raichur-Mahbubnagar, about two furlongs south of the Krishna Bridge, and three miles south-west of No. 20 above. There are about 20 stones still standing; the largest is over six feet high. All are of grey gneiss. One row of five stones survives, but I could

not recover the original plan. The orientation is 5° east of north. There is a microlithic factory site nearby and beyond it are several circle graves. The little settlement hillock at Gudabelur is clearly visible from the site. (See Plate Ja.)

38. *Piklihal I.* Not previously reported. West of the 13·6 mile stone on the Lingsugur-Gangawati road, about two and a half miles south of Mudgal. The Piklihal alignments are features of a magnificent complex of graves, stone rectangles, rock bruising and paintings, and slag heaps which surround the neolithic to Early Historic settlement, which we discovered in 1952. This alignment lies immediately south of the settlement area and adjoins stone-circle graves. There are 20 stones still standing with one row of seven stones. The orientation of this row is 20° west of north.

39. *Piklihal II.* This site is about 400 yards west of the previous one. It also adjoins the grave area. The stones are mostly conical in form and less than four feet high. It was not possible to discover the original plan, or orientation.

40. *Piklihal III.* The site is about a quarter of a mile north of the settlement in a little enclosed valley. There are about 20 stones, up to seven feet in height, of grey gneiss. The original layout was probably four rows of five and the orientation slightly west of north.

#### OTHER DISTRICTS

41. *Uppal.* Information from Sir Theodore Tasker. The site is eight miles east of Hyderabad on the Warangal road. There are at least 40 stones, but no further details were recorded.

42. *Kapracheru.* *Annu. Rep. Hyder. Arch. Dept.*, 1940-41, p. 7. Locality not mentioned but probably near Hyderabad. No further details.

43. *Cherighat.* *Annu. Rep. Hyder. Arch. Dept.*, 1940-41, p. 7. On the road from Narkatpalli-Nalgonda in the Nalgonda District. No further information.

## THE COMING OF THE ARYANS AND THE END OF THE HARAPPA CIVILIZATION

by

PROFESSOR DR. ROBERT HEINE-GELDERN

**151** The results of my earlier investigations into the present subject may be summarized as follows.<sup>1</sup>

Several types of copper and bronze objects from the Ganges and Indus plains and from the Afghan border are of western origin and indicate that some kind of migration from north-western Iran or Transcaucasia reached India between 1200 and 1000 B.C. This migration passed through the region south-east of the Caspian Sea where its traces can be recognized at Tepe Hissar near Damghan and at Tureng Tepe near Asterabad. It came from the very area where historians place the bulk of the Indo-Aryan people at the time—fifteenth-fourteenth centuries B.C.—when some of its more adventurous groups, swarming out towards the south and south-west, had acquired the mastery over the kingdom of Mitanni and parts of Syria. Therefore it seems reasonable to assume that the migration in question was that of the Aryans from the Near East to India. They may have been driven from their homes by a branch of that powerful ethnic movement which, starting from the Balkans or from the lower Danube, destroyed the Hittite kingdom around 1200 B.C.<sup>2</sup>

\* With seven text figures

When I first published these results, 20 years ago, it was still generally believed that Harappa and Mohenjo-daro had ceased to exist around the middle or, at the latest, in the second half of the third millennium B.C. Therefore a gap of more than a thousand years seemed to separate those ancient cities from the arrival of the Aryans.

In the meantime the date of Harappa and Mohenjo-daro has been revised. In the second edition of Mackay's *Ancient Indus Civilizations* their end is set around the middle of the sixteenth century B.C.<sup>3</sup> The gap was thus reduced to a few centuries. It was finally closed by Sir Mortimer Wheeler who showed with convincing arguments that the walled cities and forts mentioned in the Rig Veda as having been conquered by the Aryans were the cities and citadels of the Harappa Civilization.<sup>4</sup> However, his acceptance without proof of the fifteenth century B.C. as 'the conventional date' of the Aryan invasion of India is hardly satisfactory. The same applies to Colonel Gordon's 'estimated periods'—1800 to 1600 B.C.—for the end of the Harappan sites and the arrival of the Aryans.<sup>5</sup> In order to obtain a better founded and more exact date I have again reviewed the archaeological evidence, taking into

account discoveries made or published since my earlier papers on the subject.

A trunnion axe from the Kurram valley, near the Afghan border, belongs to a sub-type of trunnion axes known from the Mediterranean region, Europe, Transcaucasia and northern Iran (fig. 1). Wherever it occurs, that

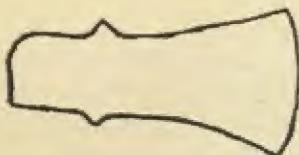


FIG. 1. COPPER CELT FROM THE KURRAM VALLEY, PAKISTAN

*Ann. Rep. Archaeol. Survey of India, 1913-1914*

sub-type belongs to the last two centuries of the second millennium or to the beginning of the first millennium B.C. The Indian specimen has its closest counterparts in some of the trunnion axes of Transcaucasia, dating from the twelfth and eleventh centuries B.C. and the beginning of the first millennium.<sup>6</sup>

A large bronze dagger from Fort Munro in the Sulaiman Range, west of the Indus, is related to similar weapons from western Iran, dating from approximately 1200 to 1000 B.C. (fig. 2). Some of the West Persian specimens



FIG. 2. (a) BRONZE DAGGER FROM FORT MUNRO, SULAIMAN RANGE, PAKISTAN, (b) BRONZE DAGGER FROM KIRMANSHAH, WESTERN IRAN

*National Museum of Antiquities, Edinburgh; British Museum*

bear inscriptions of the Babylonian king Marduk-nadin-akhé who reigned from 1116 to 1101 B.C.<sup>7</sup>

About a dozen and a half of double-edged copper swords with antennae hilts come from the Gangetic plain and from Hyderabad in the Deccan (fig. 3a-c). Irrespective of whether the leaf-shaped slashing sword originated in Europe or in south-western Asia, as far as India is concerned it is an intrusive type from the west which could hardly have reached India prior to the twelfth century B.C. The only parallel to the antennae hilts of the Indian weapons occurs in large bronze daggers of the Koban culture in the Caucasus (fig. 3d). This would seem to indicate a date between 1200 and 1000 B.C. or possibly even later.<sup>8</sup> B. B. Lal stresses the differences between the Caucasian and Indian types and thinks that it may not be justified to derive the one from the other.<sup>9</sup> However, these differences may be due merely to those of material and technique.

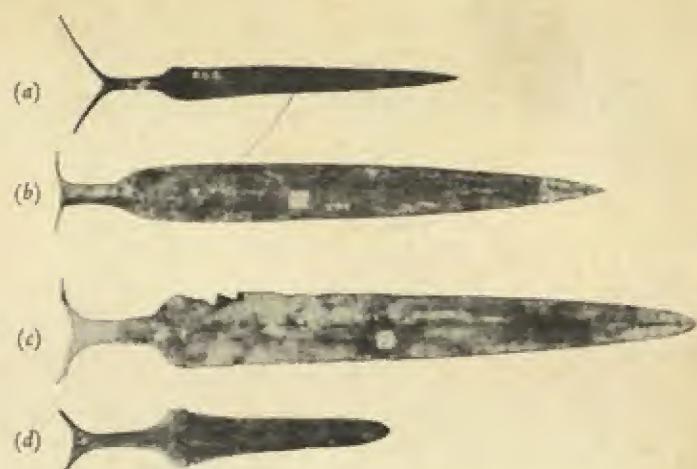


FIG. 3. (a, b, c) COPPER SWORDS FROM FATEHGOR, GANGES PLAIN, (d) BRONZE DAGGER, KOBAN, CAUCASUS

(a, b, c) V. A. Smith, 'The Copper Age and Prehistoric Bronze Implements of India,' *Ind. Antiq.*, Vol. XXXIV; (d) Countess P. Uvarova, *Materialy po Arkheologii Kavkaza*, Vol. VIII.

The shape of the Indian sword hilts corresponds precisely to what we might expect to have been the result if the Caucasian weapons had been imitated in copper instead of bronze and finished by hammering after having been cast.<sup>10</sup>

Piggott compared a copper rod from the uppermost level of Harappa (fig. 4a) to copper rods from Hissar III C and to bronze pins from Koban and Luristan.<sup>11</sup> However, the date which he assigns to it, towards 1500 B.C., is decidedly too early. The animal group forming the head of the Harappa rod, a dog attacking an antelope, corresponds to the deer-and-dog motif of Koban pins (fig. 4b) and indicates a date not prior to about 1200 B.C.

A pin from Mohenjo-daro, topped by two deer heads, may be compared to Koban pins and Hissar III C copper rods with two horse heads (fig. 4c, d).<sup>12</sup> Despite its having been found at a considerable depth it cannot be dated earlier than the twelfth century B.C. The quarrying of bricks in the Buddhist period may well have caused a small object to slip down to a lower level.

The much discussed bronze axe-adze from Mohenjo-daro (fig. 5a) was found six feet below the surface. Mackay thought that 'it is quite conceivable that it was accidentally left where it was found by a mason in search of ancient bricks for the Buddhist stupa.'<sup>13</sup> Although this supposition is unacceptable it emphasizes the late date of the object and justifies its being assigned to the last phase of the ancient city or to the very moment of its end.

The axe-adze from Mohenjo-daro belongs to the type with long tubular collar which is known from Rumania, the Ukraine, the northern Caucasus (Faskau), Assyria and Iran. This type seems to have originated in Transylvania in the first half of the second millennium B.C. From there it spread to the Near East, where it appears much later. Both Hissar III C and the contemporary 'treasure of Astrabad' from Tureng Tepe yielded axe-adzes similar to

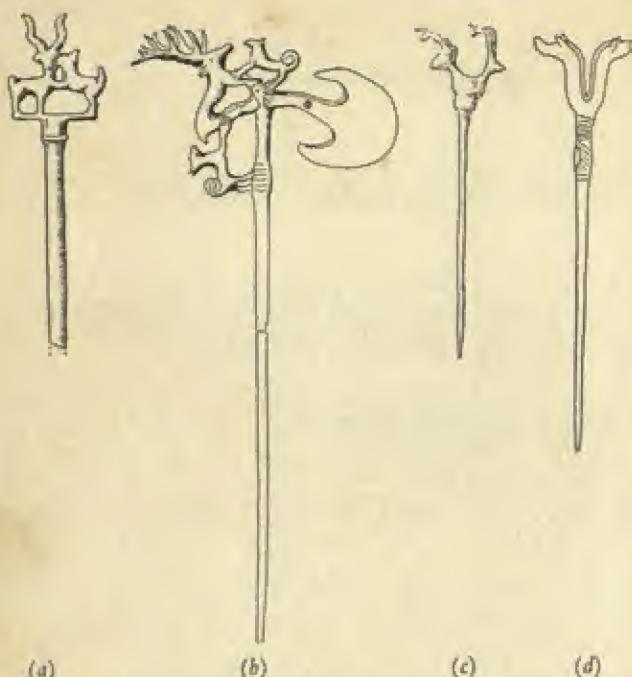


FIG. 4. (a) COPPER ROD, HARAPPA, (b) BRONZE PIN, KOBAN, CAUCASUS, (c) COPPER PIN, MOHENJO-DARO, (d) BRONZE PIN, KOBAN, CAUCASUS

(a, c) S. Piggott, 'Notes on . . . Pins . . .', *Ancient India*, No. 4; (b, d) E. Chantre, *Recherches anthropologiques dans le Caucase, Paris*, 1886

the Mohenjo-daro specimen (fig. 5b). I have dealt elsewhere with the evidence indicating that Hissar III C and the corresponding finds from Tureng Tepe must be dated between 1200 and 1000 B.C.<sup>14</sup> The similarities with the



FIG. 5. (a) BRONZE AXE-ADZE, MOHENJO-DARO, (b) COPPER AXE-ADZE, TEPE HISSAR, NORTHERN IRAN

E. Mackay, *Further Excavations at Mohenjo-daro, Delhi, 1938*; E. F. Schmidt, 'Tepe Hissar Excavations,' *Mus. J., Philadelphia, Vol. XXIII*

Koban culture of the Caucasus, the Gandsha-Karabagh culture of Transcaucasia, the Luristan culture and the cemeteries A and B at Tepe Sialk seem conclusive. Moreover, the trunnion celт found in one of the graves at Tureng Tepe<sup>15</sup> which correspond to the period of Hissar

III C cannot possibly be older than the twelfth century B.C. Consequently, the axe-adzes of Hissar III C and Tureng Tepe must be dated between 1200 and 1000 B.C. Those from Assyria are even later, having been found in a temple of the ninth century B.C.<sup>16</sup> Small bronze models of the same type occur in the Iron Age cemetery at Sialk, dating from around 1000 B.C.<sup>17</sup> The axe-adze from Mohenjo-daro thus again indicates some kind of intrusion from the west between 1200 and 1000 B.C.

A bronze mace head from Chanhudaro comes either from the final phase of the Harappa level or from the subsequent Jhukar level. Piggott correctly compared it to mace heads from Luristan and Hissar III C (fig. 6).<sup>18</sup> This

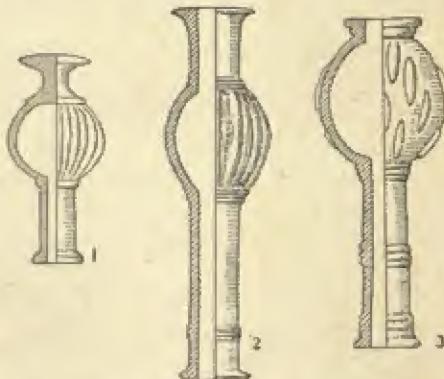


FIG. 6. BRONZE MACE HEADS FROM: 1. CHANHUDARO; 2. LURISTAN; 3. TEPE HISSAR  
S. Piggott, op. cit.

means that it belongs to the same period as the axe-adze from Mohenjo-daro.

The seals from the Jhukar level at Chanhudaro differ radically from those of the preceding Harappa culture level. Fig. 7a-f shows the close similarity of some of their ornamental or symbolic designs with those of Anatolian seals of the Hittite period. There can be no doubt that



FIG. 7. (a, b, c) SEALS OF THE JHUKAR PERIOD, CHANHUDARO, (d, e, f) SEAL AND SEAL IMPRESSIONS OF THE HITTITE PERIOD, ALISHAR HÜYÜK, ANATOLIA

(a, b, c) E. Mackay, *Chanhudaro Excavations, New Haven, Conn., 1943*; (d, e, f) H. H. von der Osten, *The Alishar Hüyük, Univ. of Chicago Oriental Inst. Publ., Vol. XXIX*

this type of seal and these designs must have been introduced by western immigrants who in their former homes had been in contact with the Hittites. We know that in the fourteenth century B.C. close relations existed between the Hittite king Shubiliuma and the Indo-Aryan king of Mitanni. Similar relations may have existed between the Hittites and the bulk of the Aryan people in Transcaucasia, a region known to have been influenced by Hittite culture.

The majority of the Chanhu-daro seals have no handles, but are perforated sidewise and were worn on a string or wire.<sup>19</sup> As Professor H. G. Gütterbock of the University of Chicago kindly informed me, Anatolian seals of this type which date from the time of the Hittite empire usually have grooved edges, while the edges of those of the post-empire period, thus roughly after 1200 B.C., are usually flat. In Chanhu-daro the majority of these seals have flat, but a few have grooved edges. This would seem to indicate that the people who introduced the type into India, whoever they were, left the vicinity of the Hittites in the transitional period, around or a little after 1200 B.C.

Gordon thinks that the people who buried their dead in the cemetery H at Harappa are 'more likely candidates for the true Indo-Aryans' than those who introduced the types of metal objects discussed in the present paper.<sup>20</sup> However, we should not forget that the claim of the cemetery H people to be Aryans rests solely on the stratigraphical position of their graves. They may have been Aryans, but we cannot yet prove it. Nor can we assign a date to their cemetery, except that we know that it is post-Harappan.<sup>21</sup>

On the other hand, is it a mere accidental coincidence that the trunnion celt from the Kurram, the dagger from Fort Munro, the swords from the Ganges plain, the animal-headed rod from Harappa, the pin and the axe-adze from Mohenjo-daro, the mace head and the seals from Chanhu-daro all point to a date between 1200 and 1000 B.C. and that they all indicate connexions with the west, with Caucasia, eastern Anatolia and western Iran? This is the very area where, on historical grounds, the home of the Aryans is believed to have been in the fourteenth century B.C. Moreover, we have indications of connexions with the region south-east of the Caspian Sea through which the Aryans must have passed on their way to India. It is significant that the axe-adzes, the animal-headed rods and the trunnion celt of Tepe Hissar and Tureng Tepe, closely related to those of India, all date from one and the same period. This shows that in northern Iran these types belonged to a single cultural complex and makes it appear likely that the same was the case in India.

B. B. Lal stresses the fact that daggers of the Fort Munro type, trunnion celts and axe-adzes were never found in the Gangetic plain, nor antennae-hilted swords and copper harpoons west of the Ganges region.<sup>22</sup> Nevertheless there cannot be the least doubt that both types are post-Harappan.<sup>23</sup> Moreover, the antennae-hilted sword is certainly derived from the west, irrespective of whether I am right or wrong in connecting it with the Koban culture. The archaeology of India, and particularly of the period in question, is still very imperfectly known. Therefore it

would be rash to draw conclusions *ex absentia*. Moreover, we cannot be sure that all groups of invading Aryans shared the same material equipment. There is even a possibility that groups of other peoples—other Indo-European or, perhaps, Caucasian tribes—joined the Aryans in their migration towards the East. This might explain cultural differences within the same movement and the same period.

There is no reason for assuming that any substantial hiatus separates the Juhkar occupation of Chanhu-daro from the preceding Harappa level. The invaders seem to have settled immediately in the ruins of the city which they had destroyed. Similarly, the axe-adze and pin from Mohenjo-daro and the animal-headed rod from Harappa, apart from proving that these cities were still in existence around 1200 B.C., indicate direct contact between them and the invaders. If the Aryans arrived in India already in the fifteenth century, as Wheeler assumes, or even earlier, as Gordon believes, who then were those mysterious invaders of the twelfth or eleventh century whose archaeological traces we have tried to follow?

The new evidence now available fully confirms my previous conclusion, i.e. that the Aryans, coming from the west, invaded India between 1200 and 1000 B.C. It also confirms Sir Mortimer Wheeler's assumption that it was the Aryans who destroyed the cities of the Harappa civilization, except that the date of that event will have to be lowered from the fifteenth century to around or after 1200 B.C.

#### Notes

<sup>1</sup> I wish to express my sincere thanks to the Wenner-Gren Foundation for Anthropological Research in New York which sponsored my research on Harappa and the Aryans. The present paper contains some of the preliminary results.

<sup>2</sup> R. Heine-Geldern, 'Archaeological Traces of the Vedic Aryans,' *J. Indian Soc. Oriental Art*, Vol. IV (1936); 'New Light on the Aryan Migration to India,' *Bull. Amer. Inst. Iran. Art and Archaeol.*, Vol. V (1937).

<sup>3</sup> E. Mackay, *Early Indus Civilizations*, 2nd ed., London, 1948.

<sup>4</sup> Sir Mortimer Wheeler, 'Harappa 1946: The Defences and Cemetery R. 37,' *Ancient India*, No. 3 (1947), pp. 81–83; *Five Thousand Years of Pakistan*, London, 1950, pp. 31f.; *The Indus Civilization*, Cambridge, 1953, pp. 90–93.

<sup>5</sup> D. H. Gordon, 'The Early Use of Metals in India and Pakistan,' *J. R. Anthropol. Inst.*, Vol. LXXX (1950), pp. 56f.

<sup>6</sup> Heine-Geldern, 'Archaeological Traces, etc.,' pp. 87–93; G. Nioradze, 'Der Verwahrfund von Kvemo-Sasirethi, Georgien,' *Eurasia Septentrionalis Antiqua*, Vol. VII (1932), pp. 87, 97; S. Przeworski, 'Der Grottenfund von Ordu,' *Archiv Orientálny*, Vol. VIII (1936), pp. 60f.; E. Dullo, 'Die kaukasischen Äxte der Bronzezeit,' *Prähistorische Zeitschrift*, Vol. XXVII (1936), p. 97; G. K. Nioradze, 'Arkheologicheskie nakhodi v sele Kvishari,' *Sovetskaja Arkheologija*, Vol. XI (1949); A. A. Iessen, 'Prikubanskii ochag metalurgii i metalloobrabotki v kontse mednobrazonovogo veka,' *Materialy i Issledovaniia po Arkheologii SSSR*, No. 23 (1951). I owe the Russian references to Dr. Karl Jettmar of the Museum für Völkerkunde, Vienna.

<sup>7</sup> Heine-Geldern, 'Archaeological Traces, etc.,' pp. 100f.; Gordon, *op. cit.*, p. 58.

<sup>8</sup> Heine-Geldern, 'Archaeological Traces, etc.,' pp. 101f.

<sup>9</sup> B. B. Lal, 'Further Copper Hoards from the Gangetic Basin and a Review of the Problem,' *Ancient India*, No. 7 (1951), p. 35.

<sup>10</sup> According to Vincent A. Smith, 'The Copper and Prehistoric Bronze Implements of India,' *Indian Antiquary*, Vol. XXXIV (1905), p. 237, the hilt of one of the Indian swords 'bears unmistakable signs of having been hammered out.'

<sup>11</sup> S. Piggott, 'Notes on Certain Metal Pins and a Mace Head in the Harappa Culture,' *Ancient India*, No. 4 (1947-48), pp. 33-38.

<sup>12</sup> Piggott, *op. cit.*, pp. 33-38; Heine-Geldern, 'Archaeological Traces, etc.,' pp. 97f.

<sup>13</sup> E. Mackay, *Further Excavations at Mohenjo-daro*, Delhi, 1938, p. 458.

<sup>14</sup> Heine-Geldern, 'Archaeological Traces, etc.,' pp. 94-99; 'China, die Ostkaspiische Kultur und die Herkunft der Schrift,' *Paideuma*, Vol. IV (1950), pp. 53-59, 86.

<sup>15</sup> Frederick R. Wulsin, 'Excavations at Tureng Tepe, near

Asterabad,' *Supplement to Bull. Amer. Inst. for Persian Art and Archaeology*, Vol. II (1932), Plate XX.

<sup>16</sup> W. Andrae, *Der Anu-Adad-Tempel in Assur*, Leipzig, 1909, pp. 53f.

<sup>17</sup> R. Ghirshman, *Fouilles de Sialk*, Vol. II, Paris, 1939, p. 47, and Plates XXVII, XCIII.

<sup>18</sup> Piggott, *op. cit.*, pp. 38f.

<sup>19</sup> E. Mackay, *Chanhudaro Excavations*, New Haven, 1943, pp. 140, 143.

<sup>20</sup> Gordon, *op. cit.*, pp. 57f.

<sup>21</sup> Wheeler, *The Indus Civilization*, p. 51.

<sup>22</sup> Lal, *op. cit.*, pp. 35-37.

<sup>23</sup> For the copper harpoons see my paper on 'Archaeological Traces, etc.,' pp. 102f.

## OBITUARY

### Clarence van Riet Lowe: 1894-1956

**152** Professor C. van Riet Lowe, whose death occurred in Johannesburg, South Africa, on 17 June, 1956, was an outstanding figure in African archaeology.

He was born at Aliwal North, Cape Province, on 4 November, 1894, and graduated in Civil Engineering at the University of Cape Town. His studies were interrupted by his service during the First World War when he served first in the South African Artillery and later with the Royal Horse Artillery in East Africa, Egypt, Palestine, Italy and France. During the recent war he commanded the battery of the Witwatersrand University Training Corps.

In his youth he had known and come under the influence of the well-known collector, 'Gogga' Brown, but it was during active service in Egypt that his real interest in prehistory was awakened. On his return he entered the Department of Public Works as an engineer and in the course of his engineering work he found many opportunities to develop his archaeological interests and began a long series of valuable contributions to the subject. This was especially so during the years 1923 to 1928 when he was in charge of bridge-construction in the Orange Free State. During this period he discovered over 300 stone-age sites along the major rivers of that Province, published numerous papers and collaborated with Professor A. J. H. Goodwin in writing *The Stone Age Cultures of South Africa* which is still the most comprehensive book on the subject. In 1931 he became Chief Engineer of the Public Works Department and so reached the top of his profession at the age of 37 years.

In 1935 the Government of the Union established the Bureau of Archaeology which later became the Archaeological Survey at the University of the Witwatersrand, Johannesburg, and appointed van Riet Lowe as its first Director. At the same time the Commission for the Preservation of Natural and Historical Monuments, Relics and Antiques was reorganized under a new Act of Parliament and he was appointed as a member and Secretary of the new body, while the University offered him a newly created Chair of Archaeology. Van Riet Lowe thus undertook the onerous task of building up three important public institutions simultaneously from their very beginnings. In spite of his heavy administrative duties in three capacities he continued his archaeological researches and new contributions came freely from his facile pen. Of outstanding importance was the survey he undertook of the gravels of the Vaal River, in collaboration with geologists of the Geological Survey, which first established climatic sequences and cultural correlations for the south of the continent and incidentally gained for him the degree of D.Sc. of the University of Cape Town, his work in the Makapan Caves in the Potgietersrust district and, more recently, a survey of the Little Caledon River in the Orange Free State. He gave much

attention to prehistoric art, of which he was a leading authority and expert copyist.

Professor van Riet Lowe's work stretched far beyond the Union of South Africa. In 1931 he represented South Africa at the Centenary Meeting of the British Association in London and visited important sites in England and France. A few years later he again represented South Africa at the International Conference on Excavations in Cairo and returned overland, studying prehistoric sites in Egypt, the Sudan, East Africa, the Belgian Congo and the Rhodesias. In 1939 he was invited by the Government of Uganda to describe the archaeology of that Territory in collaboration with Mr. E. J. Wayland. This work appeared as Part II of a memoir published by the Geological Survey of Uganda on *The Pleistocene Geology and Prehistory of Uganda* which contains the first full description of the Kafuan Culture. He twice visited Portuguese East Africa at the invitation of its Government to give advice on the organization of prehistoric research and the preservation of monuments, and took an active part in all the Pan-African Congresses on Prehistory in Nairobi, Algiers and Livingstone.

Professor van Riet Lowe's outstanding services were recognized by the award of many distinctions. He was Past President of the South African Association for the Advancement of Science, the South African Archaeological Society and the South African Museums Association, and at the time of his death was President of the Associated Scientific and Technical Societies. He was a Fellow of the Society of Antiquaries and Fellow of the Royal Society of South Africa and held the South Africa Medal, the highest award for scientific distinction of the South African Association for the Advancement of Science. His work in the interests of the preservation of historical and scientific monuments was internationally known and he was a member of the International Historical Monuments Commission of the League of Nations and of the Committee on Monuments, Artistic and Historical Sites and Archaeological Excavations of U.N.E.S.C.O. He was awarded the medal of the South African Historical Monuments Commission for his services in this field.

Notwithstanding all these honours, Professor van Riet Lowe remained a warm and friendly personality, highly esteemed by all who knew him. In him were combined the sternly practical outlook of the soldier and engineer with the vision and insight of the scientist. His death has removed one whose valuable services and guidance will be sorely missed.

B. D. MALAN

**153** With the completely unexpected death of Clarence van Riet Lowe there ends a formative epoch in the study of South African prehistory. Throughout almost a third of a century he has played a major part in the study of certain aspects of our Stone Age: first in his remarkable study of

the Smithfield Culture, which still stands as his most outstanding typological paper, then in the cultures of the Earlier Stone Age, which he knit into the pattern of changing climates as represented by the Vaal River. Not content with this, Peter (as he was called by his friends) took a remarkable interest in the prehistoric art of South Africa, copying hundreds of cave paintings and petroglyphs. He took an immense pride too in building up a collection of beads, in the hope that this might eventually permit of dating and placing outside contacts with countries bordering on the Indian Ocean. For a brief spell he interested himself in Zimbabwe, again leaving an indelible mark on the study of this subject.

In only one field did he refuse to embroil himself, that of the study of our Middle Stone Age (typologically the equivalent of the European Middle Paleolithic); here he too found the local variations too anomalous to permit of reasonable classification.

He did not confine himself to southern Africa, and his work in other areas of our vast continent, in Uganda, Tanganyika and elsewhere, will continue to have a most marked effect on the future of our subject in these areas.

With his quite unnecessary death many of us have lost a great and very dear friend, a friend of great personality and attraction. While he did not conduct a teaching course in prehistory, those students in other subjects (notably anatomy and geology) who submitted themselves to his tuition have probably benefited more from that contact than those who have undergone training under more formal guidance. He was remarkable as a teacher, a research worker, a lecturer, and in his ability to inspire other workers in the field. His ability to clarify his subject and to bring it to the level of the quite untrained amateur, concisely, briefly and accurately, was a faculty all of us envied him, and one which we shall sadly miss.

In all, we have lost a man, a man who is irreplaceable as an individual, whose work will have to be continued by a number of followers; each in a more limited field than he was able to encompass; each with a lesser ability to call upon other disciplines to help unravel the story of man's past; each acting without the stimulation of a most remarkable and lovable character.

A. J. H. GOODWIN

## SHORTER NOTES

**ABO Blood Groups in Kerman, South Persia.** By P. H. T. Beckett, *Wadham College, Oxford*

**I54**

Blood from a small number of patients in the C.M.S. Hospital in Kerman was examined for ABO groups in 1950: 41 male and 5 female subjects were from Kerman or its immediate environs, and 8 from the region south and west.

Kerman and Yezd are the only large Zoroastrian (*Zadushti*) centres remaining in Persia. There has been a negligible number of converts to Zoroastrianism since the end of the last Zoroastrian dynasty (Sassanian) as a result of religious disabilities, and often outright persecution, which have effectively discouraged the would-be proselyte; Zoroastrians do not intermarry into other religions. The present Zoroastrian population should therefore be fairly representative of that of Sassanian Persia and its composition relatively unaffected by the incursions of Arabs, Mongols, Turkomans, Afghans, etc., that have increased the complications of the pattern of racial origins in Persia during historical times.

Unfortunately local political feeling made it advisable to discontinue the work before a significant number of groupings had been made. Results were

	<i>A</i>	<i>B</i>	<i>AB</i>	<i>O</i>
Muslim	7 (3)	8 (1)	2 (0)	23 (4)
Zoroastrian	3	2	0	1

Figures in parentheses refer to Muslims living outside the Kerman area.

**Application of the Fluorine Test to the Neanderthal Skull from Gaiovce.** By Emanuel Vlček, M.U.Dr., Archaeological Institute of the Slovak Academy of Sciences, Nitra, and Jiří Pelikán, D. Chem. Sci., Archaeological Institute of the Czechoslovak Academy of Sciences, Prague.

For the further evaluation of the Neanderthal calvaria and endocast in travertine from Gaiovce near Popred, Northern Slovakia (Czechoslovakia), chemical tests have been applied to the osseous tissue, to determine its fluorine and phosphate content.

After several years' study of the locality, and mainly through the systematic research begun here in 1955, a cultural layer was found which belongs stratigraphically to the transition from the end of the mixed-oak-wood to the beginning of the conifer phase, that is to the second half of the Riss-Würm interglacial period. The human remains recently found supplement substantially the

report on the travertine endocast found in 1926 (see E. Vlček, 'Find of Neanderthal Man in Slovakia,' *Slovenska Archeologia*, Vol. I, Bratislava, 1953, pp. 5-132, and 'The Fossil Man of Gaiovce (Czechoslovakia)', *J. R. Anthropol. Inst.*, Vol. LXXXV, in the press).

In course of the detailed examination of the various palaeoanthropological finds, experimental work was applied to the osseous tissue of the Neanderthal calvaria, the chemical examination being carried out by Dr. J. Pelikán.

In order to determine the relative age of the human bones from Gaiovce a modified fluorine test was applied. The result of this test is given in the form of an index, expressing the proportion of fluorine to phosphoric acid in the bone ash. We are of the opinion that the assessment of the percentage of fluorine and phosphoric acid in osseous material from archaeological excavations is not such a simple matter as making these determinations in recent bone. In prehistoric material one must always allow for various kinds of contamination with extraneous mineral matter and for uneven fossilization. These factors can substantially affect the values gained by analysis.

For this reason we do not state the absolute percentages of either fluorine or phosphoric acid, but having determined them in all samples we express their ratios.

Our actual procedure is as follows. A suitable piece of osseous material weighing 0.2-0.5 gm. is calcined in an electric furnace for four to six hours, in order not only to burn out the organic matter but also to cause thermal dissociation of calcium carbonate, in which the bone has usually been considerably enriched by fossilization. After this we divide the calcined sample into two parts in the proportion by weight of 3:1, taking all precautions to ensure average samples. In the larger portion we determine fluorine, in the smaller one phosphoric acid. Fluorine we determine in a distillate obtained in a special apparatus based on that of Willard and Winter; following Fellnerberg the determination is made volumetrically, by cerous chloride  $\text{CeCl}_3$  on muroxide, as described by Brunischolz and Michod (*Helv. Chim. Acta*, Vol. XXXVII, 1954, pp. 598-602). Phosphoric acid we determine gravimetrically as magnesium pyrophosphate  $\text{Mg}_2\text{P}_2\text{O}_7$ , applying Complexon II (Schwarzenbach) and citric acid as shading admixtures. After checking, we express phosphorus as  $\text{PO}_4$ , and the index is calculated from this formula

$$\frac{\text{gF}}{\text{gPO}_4} = \text{index}$$

The fluorine values of the Krapina material were indicated in a similar way, but in that case the index was divided by 0.089<sup>2</sup> giving the proportion of fluorine to phosphoric acid in apatite (R. Martin, *Lehrbuch der Anthropologie*, Jena, 1928, pp. 34f.). In order to make a comparison of our results possible, the index quoted in the publication on Krapina has to be multiplied by 0.089<sup>2</sup>.

For the analysis of the Neanderthal calvaria from Gaňovce, a portion of the Os petrosum was used. For a preliminary comparison we have analysed two samples of human bone of the Bronze Age, found in the upper layer of black earth over the loess and interglacial travertine. The data gained by our analyses are as follows:

Gaňovce, Riss-Würm, sample A = 0.018631  
sample B = 0.018730

Gaňovce, Bronze Age, sample A = 0.009812  
sample B = 0.009793

We can see immediately a distinct difference between the index

values of Neanderthal and Holocene man. By means of further analyses comparison will be made with the index values of the osteological material of mammalia from all layers of the profile.

#### Horniman Museum Concerts

The following are among the free 75-minute concerts of anthropological interest to be given at the Horniman Museum, London, S.E.23, at 7.45 p.m. on Wednesday evenings during the autumn of 1956: 17 October, 'Folk Dancing, Folk Songs and Instrumental Music of India,' arranged by Mrs. S. Warren; 24 October, 'Design for Dancing,' by the Caribana's Steel Band from Trinidad; 31 October, 'Music of Persia,' by members of the Asian Music Circle; 7 November, 'Folk Songs, Guitars and Drums from Many Lands,' by Miss Victoria Kingsley; 21 November, 'Folk Music of Ireland,' a recital by Seamus Ennis, Irish piper and singer. Seats may be reserved at any of these concerts on application to the Curator, Dr. O. W. Samson.

## REVIEWS

### AFRICA

**Préhistoire de l'Afrique.** By H. Alimen. Paris (Boubée), 1955.  
Pp. 578, 25 half-tone plates, 3 colour plates, 155 text figs. and  
157 5 tables

Almost the only concise account of African prehistory that has been available up to now has been Dr. Leakey's *Stone Age Africa*. As this was published in 1936 and much new material has been brought to light by systematic investigations in many parts of the continent since that date, there was a great need for an up-to-date general introduction to African prehistory. Mlle. Alimen's book goes a long way to filling this need, providing as it does an easily readable account in a useful size of the present state of our knowledge in the different parts of the continent which will be of use to all seeking a concise outline of the African prehistoric cultures.

The book is divided into thirteen chapters of which the first nine are devoted to describing the cultural and climatic succession in the main regions and summarizing the findings from each area, namely French North Africa (the Maghreb), the Sahara, Tripolitania and Cyrenaica, Egypt, the Horn, East Africa, West Africa, the Congo Basin and South Africa. In the remaining chapters the author reviews discoveries of fossil man, rock art and the megalithic complex and in the final chapter sums up and gives her conclusions. A bibliography is given of the main works consulted for each chapter.

The first chapter is an excellent summary of the French North African succession and sets out in admirable manner the stratigraphic and climatic succession as based on marine strand lines and continental deposits. Into this framework is set the cultural succession and good descriptions and illustrations of the cultures are given. The accompanying text figures of sections and distribution maps greatly enhance the value of this section. The same can be said of the chapters on Egypt and the Nile Valley and that on the Sahara which admirably summarize the evidence from each area and attempt to interpret the inter-relationships between the different cultures of the two regions and their connexions with those in the Maghreb. The book was printed before the results of McBurney's and Hey's work in Cyrenaica or of the writer's in the Horn were published, so that these areas are only briefly dealt with. In the chapter on East Africa due stress is given to the difficulty of interpreting 'pluvials' and 'interpluvials' in areas where volcanic activity has caused earth movement often on a considerable scale. The description of the East African cultures could have been more complete and one must deplore the continued use of the word 'Mousterian' to describe the proto-Stillbay industries of the Kenya Rift.

The new terminology adopted for the Congo Basin cultures by

the 1st Pan-African Congress on Prehistory in 1947 has not been used in the description of the prehistory of that region, so that it is still sometimes difficult to correlate the succession there with that in other parts of the continent to the east and south, especially as fully illustrated descriptions are still lacking for several of the cultures there.

One would have preferred that the sub-continent south of the Congo Basin had been subdivided into at least the two obvious subdivisions, namely the cultures of the tropical savannah and those of the grasslands, sub-tropical forest and *fynbos* to the south of the tropical belt, rather than that the whole of this area should be treated as a single ecological unit which it certainly is not. The author has, however, tried to compress the description of the whole of this area into one chapter with the result that only the bare succession emerges. Fuller treatment and additional illustrations would have been desirable, nevertheless the essentials are there although much of the recent work done since the war and published, for example, in the Bulletins of the South African Archaeological Society, has not been included.

Small details are not always correct. For example, a reference to tectonic movements in the Gambian has crept into the Table at Fig. 86 showing the climatic and cultural succession in Northern Rhodesia. F. Malan's description of the Stellenbosch industry in the Wagtemakers Valley has been ascribed to B. D. Malan, P. G. Brien's work in Natal has been credited to T. P. O'Brien and Table IX shows the Rhodesian cultural succession closing with a Neolithic which has long since been resolved into thin air.

A concise and factual account is given of the Australopithecines in the chapter on fossil man and this is followed by a summary of the main discoveries of fossil man in the continent. Again one would have liked to see more illustrations. For example, the most widely spread form in the Middle Stone Age in South Africa is the Boskopoid type but this is not illustrated. There is no mention of the recent discovery of the Rhodesian-type man from Hopefield. The Florisbad skull has rather surprisingly been given racial status.

The rock art of North Africa is very well summarized in the chapter devoted to prehistoric art and there is an interesting distribution map showing the various art groups.

In summarizing her conclusions the author has found the same difficulties in correlating the climatic and cultural successions in the different regions as have other writers in this field. Of considerable interest are the seven distribution maps of cultures and rock art which will be found to be of considerable value. However, a word of caution must be sounded as it is possible to read more into a distribution map than is warranted by the facts. For example, the

presence of some half-dozen or so genuine Pre-Chelles-Acheul sites in South Africa and even less true Chellean ones does not warrant almost the whole of the sub-continent being shown as a province of these cultures, and it might have been preferable to have indicated only the individual sites.

The book is completed by a number of half-tone plates showing the main tool forms of the different cultures, human fossils and rock paintings and engravings.

It is a book that students of African prehistory will find useful as an introduction, providing a good general outline of their subject, while there is much, especially in the North African chapters, which will be of value to the more specialized research worker. The author has given us a valuable summary of what has now become a most voluminous research literature and for this we are in her debt.

J. DESMOND CLARK

**Funeral Dirges of the Akan People.** By J. H. Nketia. Achimota (Univ. Coll. Gold Coast), 1955. Pp. v, 296. Price 13s. 6d.

**I58** This is an excellent little book for which Mr. Nketia and the Department of African Studies of the University College of the Gold Coast, in which he is a Research Fellow, deserve congratulations and gratitude. Mr. Nketia, himself an Akan, has not only made a large, interesting and representative collection of dirges sung by Akan women at funerals and post-funeral celebrations, and reproduced them in Twi and Fante with English translations together with musical transcriptions, but also he has written a scholarly dissertation on their themes, their language and their literary and musical significance; above all he has made a sustained and successful effort to describe and explain the dirges, always in the context of their social use.

There is a full and dignified description of the traditional observances at the time of death and at funerals. Funeral singing is performed only by women, as 'wailing does not become a man.' Each woman is free to express her grief in song as she will, but the forms are stylized and certain traditional themes connected with the matrilineal clan (*abusua*) and the patrilineal ritual line (*ntoro*), the name of the deceased, his place of domicile, or fairly stereotyped expressions of sorrow and farewell recur. Discussion of these themes leads at once into discussion of the significance of clan and *ntoro*, of personal names and place of domicile for the Akan. Mr. Nketia deals with each of these briefly but clearly, never losing sight of the fact that the funeral dirges are expressions of social reality and not mere literary exercises. Such close and continuous relation of the symbolism of poetry and music to its social context is as valuable as it is, unfortunately, rare.

In dealing with the *ntoro* (patrilineally inherited ritual status) Mr. Nketia has not been too careful in his use of terms. By describing the members of the same *ntoro* as a 'group' he tends to perpetuate the mistaken belief that the Akan have a system of double descent. From his own text, however, it is quite clear that Mr. Nketia does not think of the *ntoro* as a 'group' in any organized, sociological sense. For the sake of clarity it might have been better to have distinguished unequivocally the corporate groups, which are the Akan matrilineages and matri-clans, from the unorganized but culturally distinguishable categories, which are the *ntoro* lines of patrilineal descent.

Not only is the study of the themes of the dirges closely related to a study of their social context, but so also is the study of their literary and musical styles which are critically explored. Not the least interesting part of the book is the description of fieldwork, the evident interest that the enquiry aroused and the revelation of the aesthetic excitement the Akan people find in their own literary and musical forms.

The book is written in a straightforward, workmanlike prose which, unlike that of many anthropological works, is a pleasure, not an effort, to read.

BARBARA E. WARD

**The Kongo, Vol. I. Studia Ethnographica Upsaliensia, Vol. IV.** By Karl Laman, with preface by S. Lagercrantz and B. Sundkler. Stockholm, 1953. Pp. viii, 155, text figs. Price £3

**I59** This book, to be followed by two other volumes, contains only a portion of the unpublished MSS. left by the late

Karl Laman, the Swedish missionary. The editor, Dr. Lagercrantz, warns us of the obvious defects of the book; his comments will be published in a final appendix.

Under the rather too general name of 'Kongo' the author gives us a more or less coherent series of observations on the Sundi (Basundi) group, which the Portuguese believed formed a province of the Congo Kingdom. The main interest of these notes lies in the fact that they were made in the field between 1891 and 1919. These dates also explain the inadequacy of the notes—the author did not know the sociological technique which is today the basis of all serious research.

Nevertheless, this book brings us further information on a group belonging to the important culture of the old Congo Kingdom. The author recalls that the Bakongo-Basundi, as a group, were established between Stanley Pool and the Atlantic; unfortunately he does not define the exact territory of the Basundi at the beginning of this century. Father Van Wing, who studied the Mpangu group, places the Basundi south of the river, and west of the Inkisi river (*Études Bakongo*, Brussels, 1921, map at end), while the latest map of Mlle. Boone ('Carte ethnique du Congo belge et du Ruanda-Urundi,' Zaire, May, 1954) on the other hand, places them on the northern bank. Laman writes that in the Portuguese period the Basundi lived on both banks, in the region between Matadi and Inkisi. According to the author, only a few Basundi clans remain on the south bank, where they are indigenous: long before the foundation of the Congo Kingdom, he suggests, they extended widely on the north side of the river, forming an autonomous state.

The greater part of this first volume is devoted to the history of the Basundi. On this subject, the author relies too much on the writings of Torday and Joyce, the superficial character of which M. Vansina and I have been able to establish during separate researches on the Kasai. Unfortunately, Torday's work sometimes relies on grave errors of information.

Laman follows the hypothesis of Torday and Joyce in which the Bushong (Bushongo or Bakuba) came from Lake Chad; like them, he believes that the Bushongo and the Bakongo were formerly one and the same people. On this count, we must emphasize that the proposed linguistic affinity between the names Bushongo and Bakongo is perhaps acceptable, but that in any case one may regret that Laman did not discuss this point from the linguistic point of view, however briefly. He simply says that 'k frequently becomes s,' which is somewhat inadequate. He then concludes too hastily: 'The Kongo thus immigrated from the Shari to the Kasai province about 550 A.D.' (p. 10).

The Basundi claim descent from a female ancestor, Mansundi, whose name is also given to the paramount chief. Descent is matrilineal among them as among the Mpangu studied by Father Van Wing; in both tribes the clan has the same name: *mwila* (Laman, p. 12; Van Wing, p. 118). But further on, the author seems to use the term Mansundi for members of the Basundi tribe: the Mansundi, he suggests, know of 12 female ancestors (p. 15); the genealogy which is considered to illustrate this statement seems hard to follow.

The author also states that the first Basundi branch is called 'male,' while the second is called 'female'; this calls for some elucidation. But the author offers no comment.

Information of a sociological kind is presented in no very clear order among the somewhat confused historical notes. One curious trait may be noted: the Mansundi people are forbidden to eat leopard flesh; here the leopard appears at the same time as the symbol of power—investiture takes place on a leopard skin—and as the 'brother' of men (p. 16). The feline may be killed when it has done wrong: there then unfolds a curious rite intended to remove the animal's chiefly dignity; this may perhaps be interpreted as a rite of 'desacralization.' The history of the old clans is traced rather vaguely, in language unfortunately encumbered with native words that add nothing and are not always clearly defined. Also, the historical method of the author should have been explained; and it is very difficult to follow the account without a map.

There is no obvious plan governing the composition of this work. After the long historical introduction, the chapter on 'Inhabitants'

includes notes on physical anthropology as absurd as this: 'The body odours are almost intolerable for Europeans, but they vary considerably.' The same chapter includes further vague and uninteresting notes on the 'friendship' existing between the members of one 'family' or 'clan'; the subsequent volumes will perhaps bring us more definite information on social and family structure, but one may still regret that this information does not form the very framework of the first volume. Before presenting the table of cultural values, the author expresses simple wonder that there is no 'word or expression really adequate to describe love, chastity and purity' (p. 47).

The following chapters cover Food; Care of the Body; Housing; Means of Subsistence; and Occupations. These pages will have particular interest for the specialist in technology, and are copiously illustrated with good drawings. Special mention may be made of the notes on the metallurgy of iron, copper and lead. The Basundi used to mine all three of these metals, but even by the beginning of this century, a great number of old mines had been abandoned. Copper, like lead, was poured into sand moulds to form ingots which were sold in the market; they were then worked into bracelets whose function the author unfortunately does not define. Bronze was a European import.

The author notes, and illustrates with a drawing, a form of crossbow among the Basundi.

**Les Dinariques: leur origine.** By Étienne Patte. Paris (Hermann), 1953. Pp. 98. Price 400 francs

**160** This little book represents an attempt on the part of a well-known member of the continental ('racial type') school of physical anthropology to express his dissatisfaction with some commonly held concepts—'L'anthropologie à la Tour de Babel et son Labyrinthe où une excursion est salutaire.' It would be easy to dismiss it on the grounds that Étienne Patte attempts to apply principles derived from genetics to characters of which the genetic basis is too little known, that too little account is taken of environmental effects on head characters. But such a course would do the author an injustice, for there is much serious thought behind the work despite its weaknesses.

The book opens with a study of how the definition of diverse 'races' in Europe has come about. Professor Patte points out that since extreme characters attract the most attention, what is thought to be representative may in fact be a caricature; that to designate an individual as a 'survival' of a particular type is illogical; that on maps of single characters, areas of maximum frequency do not always correspond to centres of dispersion. He suggests that maps of the frequency of individuals possessing particular combinations of characters may be more useful than those dealing with individual characters, especially when the expected and observed frequency of particular combinations can be compared; reasons for any discrepancy between observed and expected frequencies are discussed.

There follows a comparison of several European cranial samples. In former times there appears to have been a different geographical distribution of individuals possessing the Dinaric combination of characteristics. Whilst the possibility of appearance of brachycephaly on many occasions by mutations hinders the search for the origin of any given brachycephalic type, there are found right from the Mesolithic Age human types capable of producing, by recombination of their features, the Dinaric morphology and also that characteristic of the Beaker folk. True Dinaric types, though few in number, certainly appear in western Europe in the Chalcolithic and Bronze Ages; selection may subsequently have led to their multiplication. Brachycephaly did not apparently originate in the present Dinaric area. It preceded the planoccipital morphology in West Asia and seems to have been especially associated with the Chalcolithic Bellbeaker culture, perhaps following the Bellbeaker migration route traced from Spain.

The book is not easy to read. It is somewhat diffuse, and is intentionally without constructive conclusions, for Professor Patte has tried to draw attention to some aspects of the problems rather than solve them.

D. F. ROBERTS

The last pages have a great number of interesting remarks on commerce and the market, which are under the jurisdiction of a grandson of the chief. The medium of exchange was raphia cloth; it came in bundles of 4, 10 or 12 pieces. Here we may note that this old currency has always been used north of the Kasai, among the Basongo-Meno, belonging to the Bankutshu group: I visited them in 1954 and saw them using four separate pieces of cloth rolled up in leaves, as a monetary unit for bridewealth or to gain prestige.

The author notes also that blue glass beads strung into necklaces were introduced by the Europeans as currency; these necklaces were called *nsanga*. Yet the terms *osanga* among the Batetela of the Kasai (de Heusch, 'Valeur, monnaie et structuration sociale chez les Nkutshu (Kasai, Congo Belge)', *Rev. de l'Inst. de Sociol.*, Vol. 1 (Brussels), 1955) and *musanga* among the Balega of Manyema (D. Biebuyck, 'La monnaie musanga des Balega,' *Zaire*, July, 1953) mean a string of mollusc-shell discs.

The posthumous notes of Laman (who was already distinguished for his linguistic researches) certainly deserve publication, but the editors could perhaps have tried to introduce better order into the manuscript. I earnestly hope that they will not hesitate to do so when they offer us the rest of the documentation left by the Swedish missionary.

LUC DE HEUSCH\*

\* Translated from the French by Miss M. A. Bennet-Clark

## EUROPE

**Stonehenge.** By R. J. C. Atkinson. London (Hamish Hamilton), 1956. Pp. xv, 210, plates, line-drawings, maps. Price 16s.

**161** A reviewer can have little but praise for this latest addition to the literature on Stonehenge, which, as Mr. Atkinson himself points out, exceeds a thousand separate treatises. So valuable is this book, its contents so objectively described and argued, that it can safely be said to supersede everything that has been written about Stonehenge; its only critics will be the author and his colleagues, whose research work there is not yet completed.

We now see that Stonehenge was erected in three separate stages, each with its sub-divisions. The unravelling of these periods constitutes the major part of the book. Stonehenge I consisted of an encircling quarry-ditch providing material for a large inner, and a smaller outer, bank. An entrance through this earthwork gave access to a circular area containing a setting of 56 ritual pits, with the likelihood of a timber structure at the very centre. Immediately outside the entrance stood a single undressed sarsen, the Heel Stone, together with a timber structure and two other stones representing some sort of gate.

In the next phase the entrance was enlarged, an avenue of two parallel ditches dug, leading eastwards to the River Avon at Amesbury, and up this were brought 82 bluestones of Pembrokeshire origin, to be set up as a double circle with elaborate entrance, at the centre of the earthwork of phase I. Atkinson suggests that at this time the ditch around the Heel Stone was dug. Dating evidence, scant as it is, shows that these two phases belong to the first half of the second millennium B.C.—our late Neolithic period.

Phase III, with its three clear sub-divisions, provided in the Early Bronze Age substantially what we see today. In phase IIIa the stones of phase II were demolished and, after a surveying operation (commemorated in the four Stations) to find the exact centre, a gigantic setting of sarsen stones from the Marlborough Downs was erected, an outer circle with a continuous ring of lintels, and an inner horseshoe of 3 trilithons—pairs of uprights each with its lintel and graded down in height from the centre. In phase IIIb, so far purely hypothetical, 22 of the best bluestones were dressed carefully to shape and set up in some form within the sarsen horseshoe; this must have incorporated at least two bluestone trilithons, while two more bluestones were erected edge to edge with a tongue-and-groove joint. The remaining 60 undressed bluestones were planned to be set up in a double circle outside the lintelled sarsen ring, and the Z and Y holes were dug to receive them. The Altar Stone, of Pembrokeshire sandstone, was probably added at this time. Now came a radical change of plan. The Z and Y holes were abandoned (though token

chips of bluestone were dropped in each) and the 82 bluestones were set up within the sarsen circle—a horseshoe of uprights within the sarsen trilithons and a circle of uprights between the two sarsen structures.

Thus stood Stonehenge, the most elaborate ceremonial monument in Bronze Age Europe. Around it were to cluster the burial-mounds of the chief people in prehistoric Wessex; on its stones scores of carvings were to be made, including a dagger, axes and other symbols; and it was to draw to itself, inevitably, those myths and folk-tales so well described in the last chapter.

These events have been described objectively and brilliantly by Mr. Atkinson in the first half of his book. Then follow chapters in which his very great ingenuity and his scholarship are delightfully combined with an unwonted imagination to provide his readers first with a discussion of the techniques involved in the transport, dressing and erecting of the different stones, then with a survey of the cultural setting of the monument and finally with some interpretations of its various structural features.

The newest things in this book include the discovery of phase II, the hypothetical phase IIIb and the carvings. There are many details we should know but tend to forget—the dishing of the tops of the sarsen uprights, chamfering of lintel-bases against lateral movement; and the small outer bank to phase I.

Among minor points of criticism, the case for a phase II date for the ditch around the Heel Stone is not convincing; the suggested table of dates for the building sequence is perhaps too uncertain to publish. I think the arguments for Mycenaean inspiration are generally overdone. And I am sure it would have been perfectly possible to lay out accurate circles for the Z and Y holes after the sarsens had been set in position.

The book is enhanced by a superb set of Mr. Atkinson's photographs, by some interesting line drawings (though some have suffered from over-reduction; and why no plan locating the carvings?), by a bibliography and a comprehensive index.

NICHOLAS THOMAS

**Terracotta Figurines from Kourion in Cyprus.** By J. H. Young and S. H. Young. Philadelphia (Univ. Mus.), 1955. Pp. 260, 74 plates, text figs. and maps. Price \$5.50

**I62** This study of terracotta figurines is the first of the definitive publications of the prolonged excavations of Pennsylvania University at Kurion. It is concerned with a large quantity of small votive figurines from two deposits in the temple of Apollo Hylates; one the 'Archaic Fill' covering the late eighth-early fifth century, and the other the 'Votive Deposit' covering the sixth century B.C.—A.D. 100 together with a small amount of material from other parts of the site. These groups, ranging over some 800 years of the history of votive offerings to Apollo Hylates, yield an interesting picture of the changes in fashion during that period.

The earliest dedications are of bulls, some with snakes climbing up the legs, relics perhaps of the Late Bronze Age cults; and with them are votaries playing cymbals and bearing animal offerings. At the end of the seventh century there is an abrupt change to the dedication of horsemen, commonly associated with Apollo throughout the Greek world. These and chariot groups supplant other forms of votive objects down to the end of the sanctuary deposits.

Many of these types of figurine are already well known from Cyprus, but for the first time we have an intensive study of a great bulk of broken material which needed prolonged study and sorting. It has been dated stratigraphically by sherds found with the figurines; but with regard to comparison with types from other sites, it has been found that the figurines are highly localized in manufacture. But the development of the types themselves gives a useful sequence. These are set out in a compressed catalogue followed by discussion of the manufacturing methods. The transition from wheel-made to solid hand-made figurines is clearly defined; and the face mould replaces the wholly hand-made figurine at the end of the sixth century. The study of the original moulds and prototypes, and the method of taking casts to produce further prototypes forms one of the most interesting deductions, especially with regard to mould shrinkage, and the adding of special features.

Dress is chiefly represented by the development of headgear, as

the body forms are very simple. In a few cases weapons and shields can be distinguished, some of the latter with devices as a Gorgoneion.

The authors are to be congratulated on a well produced study of an important series of figurines, and their work will set a standard for future comparative material in the island. The photographs are good, though the collotype reproduction leaves something to be desired. The simple line drawings are useful for defining the details of type.

JOAN DU PLAT TAYLOR

**Le deuxième Cimetière avare d'Ullö.** By A. C. Sos. Recherches anthropologiques sur les Ossements avares des Environs d'Ullö. By P. Lipiak. Acta Archaeol. Acad. Sci. Hungar., Vol. VI. Budapest, 1955. Pp. 193-230, 231-316

**I63** Two eighth-century A.D. Avar cemeteries in Pest country yielded many skeletons allowing detailed measurements of 69 male and 58 female adult skulls. A gracile dolichocephalic element, somewhat more numerous among the men, contrasts with a markedly brachycephalic element, tentatively described as Mongoloid, somewhat more numerous among the women. Did the Avar migration bring its women with it, perhaps under pressure from eastern neighbours? The grave goods suggest diversity of clans and their customs.

H. J. FLEURE

**Atlas aérien, Vol. I: Alpes, Vallée du Rhône, Provence, Corse.** By P. Duffontaines and M. Jean-Brunhes Delamarre. Paris (Gallimard), 1955. Pp. 192, 209 plates. Price 2,500 francs

**I64** This volume aims at illustrating Human Geography by means of air photography. It is an excellent method, and the results are stimulating. The 'plan view' of Humanity from the air can usually add considerably to what we can see on maps, not only in deserted regions but also in those which have been elaborately cultivated and settled. The value of air photography to archaeology is famous; 'ancient landscapes'—whether buried below ground, or still preserved by surface traces which call for the 'total mapping' of the small topographical details—have been recorded for the first time in many countries by this means. But the volume under review has little to contribute on this subject. Instead, its emphasis is placed on demonstrating the relation between the man-made landscapes of the present day and the geographical setting supplied by nature.

The air views show the imprint of Man's handiwork in the town-plans, field-patterns and types of land-utilization in these French lands in and near the Mediterranean. The numerous illustrations, well reproduced (many of them from the air-photograph library of the Institut Géographique National), serve to show the geo-physical facts which supply a material foundation for the peasant communities of these regions. The natural resources of pasture and forest, the uncultivable mountain slopes, the intensive and exacting discipline of vine cultivation—all these and similar factors are fundamentals in the life of villages, families and individuals. The comprehensive aerial view, particularly the vertical view, is in a true sense a pictorial document of first-rate value in sociological and anthropological studies. To illustrate this approach, I have in recent years given several sets of lectures in the University of Oxford, with the title 'Air Photography and Anthropology.'

In this volume each photograph is supplied with a commentary, and an introduction discusses the method of study and the basic environmental facts.

JOHN BRADFORD

**Le origini del teatro italiano.** By Paolo Toschi. Turin (Edizioni Scientifiche Einaudi), 1955. Pp. xiv, 767, 81 illustr.

**I65** It took Professor Toschi, of Rome, 30 years to analyse critically the wealth of relevant folklore which has been published since the second edition of Alessandro D'Ancona's standard work of the same title was printed in 1891. In view of this tremendous task it is particularly gratifying that, unlike other continental folklorists, Professor Toschi by-passed neither the literature written in English nor the problems which confront us here. The author's theories on May festivals, Robin Hood, the Morris dancers,

folk songs, etc., deserve the careful attention of scholars specializing in these various subjects.

The origin of the Christian drama goes back to the annual repetitional cycle of the principal events in the Life of Christ. Schools and churches gave birth to the anonymous mediæval drama and the share of young men's associations (*Männerbünde*) is emphasized. The beginning of the profane theatre, however, is attributed to religious rites which survived from the pagan world. Professor Toschi shows that the main characters of the stage as well as of the great festivals carried on the ancient ceremonies: driving out the evil, preparing a rebirth by purification and collective confession of sins, and thereby propagating the fertility of the soil and the prosperity of the people. In the second chapter the author discusses the various spectacular forms of the rites: procession, lyric chants, the underlying myth and the dance. The separation of procession and dance seems to me detrimental to their close association.

The unbroken tradition of Italian festivals and plays enabled the author to state that the comedy stems from the licence of the carnival and the drama derives from the spring ceremonies, *Calendimaggio*. Originally both the comedy and the drama had a sacred character. Professor Toschi reminds us that the tragedy

appeared only during the middle of the *cinquecento* in imitation of the classical tragedy.

In the sixth chapter we find the first survey of carnival masks from the various regions of Italy. The choice is wisely restricted to those which can also be found in the *commedia dell' arte*: e.g. harlequin, zany, Pulcinella. The devil of the sacred drama is, of course, also included. As regards the original demonic character of the masks the author agrees with the interpretation given by Professor K. Meuli, of Basel.

Professor Toschi maintains that in Italy the society as a whole had its share in the theatre, that town dwellers and country folk were specially well acquainted with one another and that poets had a particularly deep insight into folk life. This seems an oversimplification which is bound to rouse some criticism from the Central European school of folklorists which classifies folk plays according to their relations to the various social classes.

Whereas we regret the omission of a subject index, we admire Professor Toschi's inspiring and yet accurate style, his ability to make good reading out of a staggering amount of detailed information and to withstand the temptation of making his *magnum opus* more voluminous.

E. ETTLINGER

## OCEANIA

**The Australian Aborigines: How to Understand Them.** By A. P. Elkin, Sydney (Angus & Robertson), 1954. Pp. xvii, 343. Price £1 10s.

**166**

This is the third edition, greatly enlarged and revised, of a work which has justly met with world-wide acclaim (even to a Moscow edition in 1952). The author's unrivalled field experience, his wide study, and his activity as initiator of so much research over some three decades, provide him with a vast array of material which he skilfully marshals and compresses. Some of the formulations and analogies may perhaps strike the academic reader as rather simplistic, and the general introduction inevitably raises a hundred and one issues which one would like to debate, but the book is intended as a popular introduction to aboriginal society.

The chapters on kinship and marriage summarize valuable material, with a theoretical approach which is basically that of Radcliffe-Brown's original monograph. The chapter on social groups leaves many questions of the structure and interrelations of political and local groups still to be solved.

But it is the expanded chapters on ritual, cults, religious doctrines and philosophy which are of special interest, as they incorporate much new and fascinating material on these beliefs and practices. Entirely new chapters on art, music and dancing are also useful products of recent research, though they suffer somewhat from discussion in commonsense 'layman's' terms of highly specialized fields (e.g. the vague remarks on polyphonic music, harmony, etc.).

The general theoretical cast of the work is that of a functionalism which is often an almost Smutsian holism; this hardly commends itself at a time when an irreverent younger generation of anthropologists (and many older ones!) are looking for a more adequate theoretical approach. Added to this is a heavy emphasis on the religious aspects of aboriginal culture—often in terms of aboriginal subjective categories—which is presumably a special interest of the author (cf. the predominant stress upon the ritual aspects of art, and the pleas addressed to missionaries).

This weighting is evident in the plan of the book: five chapters out of the total thirteen discuss totemism, the secret life, initiation, aboriginal philosophy, rites and beliefs, medicine-men and magic, 'death and what follows'; three cover the family, social groups and

'puzzling' kinship and marriage customs; two cover art, music and dancing.

The many complex unsolved problems of political, legal and economic organization—the worst gaps in aboriginal studies—are partly treated in Chapters II, IV and elsewhere, but receive no treatment comparable with that accorded to the questions just mentioned.

The general approach is one at the level of generalized custom with illustrations by diverse examples. This is, of course, largely appropriate to a general book of this kind, but one is constantly left wondering just how these customs work out in the stress and strain of daily life, what happens in specific situations of sorcery accusations or of feud. One longs, in reading the absorbing material on inquests, for some concrete material analogous to Evans-Pritchard's analysis of Zande oracles. But if such material is not summarized in this book, it may be because Australian ethnography has been conspicuously deficient in this kind of research. These features may well be partly a product of a situation in which, in fact, most aborigines are no longer leading a nomadic tribal existence. It is difficult, nowadays, to study indigenous political and legal mechanisms when most aborigines are in rural employment or reside on mission stations or other settlements.

But art, ritual, etc., can still be studied. This may well explain the particular emphasis of this book, but it would be misleading to accept the work as a picture of the life of most aborigines. It deals with a rather indeterminate 'ethnographic present' which the newly-added Epilogue on the history of Black-White relations contradicts. This addition is, however, a valuable, if rather sanguine, section to the rest of the book.

There are many new and valuable insights in this work which will no doubt be followed up: the effects of environment upon social grouping (pp. 46-7) and upon art forms (p. 241); the important discussion of matrilineal 'social' clans and patrilineal cult clans; the locations and interrelations of the various cults found in Arnhem Land and adjoining regions and of the totemic and sky cults elsewhere; changing marriage and initiation practices, etc.; much of this is discussed in developmental terms. For all these and many other contributions, Professor Elkin's book is most welcome.

P. M. WORSLEY

## CORRESPONDENCE

**The Cruck Truss.** Cf. MAN, 1956, 107

**167** Sir.—Sir Cyril Fox and Lord Raglan (*Monmouthshire Houses*, vol. I, p. 50) refer to 'the (probably prehistoric) cruck-trussed wattle-walled house... modified in the last phases of its history.' One of these authors, Lord Raglan, now sug-

gests 'that in the thirteenth century the cruck truss was devised.' In his second thoughts about this problem, Lord Raglan ignores the Welsh evidence for an earlier date. The laws of Hywel Dda (died 950)—of which copies dating to about 1180 (in Latin) and 1200 (in Welsh) are preserved—refer unmistakably to crucks

(*neifffyrch*). Nor can I believe that the Gothic arch would be copied in wood in Harlech Castle as early as 1278 when a bakehouse was built there with two great crooked beams 'called *crokkes*' 25 feet in length. Similar *postes torti* are among the timber supplied at Kempton (1233) and Windsor (1236) (L. F. Salzman, *Building in England down to 1540*, pp. 194-7).

My statement that cruck technique is a feature of building construction throughout north-west Europe does not depend merely upon Erixon. Halvor Vreim illustrates a primitive building with curved crucks in Finnmark (*Norsk Træarkitektur*, p. 1). The well-known Frostvikens (Jämtland) crucks are curved (*Nordisk Kultur XVII: Byggnadskultur*, p. 295). Aage Roussel in discussing a house at Caithness (*Norse Building Customs in the Scottish Isles*, pp. 45f.) agrees with H. Zangenberg's description of this Norse building custom: 'The posts, vertical at the lower end, bend inwards higher up till they meet at the top, where at their intersection they form a bed for a slender ridge . . . [This construction] in Scandinavia seems to have been mentioned for the first time by Olaus Magnus in the sixteenth century under the name of *arciales*.' Dr. Ake Campbell has told me of curved crucks in Lappland. Some of them are illustrated in Dr. Sigurd Erixon's great work *Svensk Byggnadskultur*, pp. 54, 81. Cloppenburg Museum in Germany exhibits a fine example of a building with curved principals. In short, we are not 'back at Teapot Hall'! But even if we were, it has not yet been established that some 'straight crucks' were not earlier than the curved. Some were undoubtedly later, some earlier. It seems, however, particularly fruitless to argue that a cruck's shape can always be relied upon for dating it or that one shape is derived from another. The essential fact is that, whether curved or straight, the inverted V performed one and the same function, that of holding the roof. In view of all the evidence, I cannot believe that it had any connexion with the Gothic arch. I believe that the solution lies in the direction indicated by Sir Cyril Fox and Lord Raglan in 1951, that the cruck truss is 'probably prehistoric.'

St. Fagans

IORWERTH C. PEATE

#### Arrangements of Stone Blades in Yorkshire and Arabia. Cf. MAN, 1955, 145

**168**

SIR.—A passage in Dr. H. Field's article 'New Stone Age Sites on the Arabian Peninsula' (MAN, 1955, 145) seems to be of interest in connexion with one of the minor puzzles of British prehistoric archaeology.

In a paper in the *Antiquaries' Journal*, Vol. LV (1924), p. 47, Mr. F. Buckley described the discovery at White Hill, near Marsden (Yorkshire), of 35 microlithic blades lying in a straight row; they were placed at intervals of from one and a half to three inches and the whole row covered a space of approximately six feet. This arrangement not unnaturally suggested to Mr. Buckley that the blades represented the surviving part of a composite implement, having been originally inserted in some kind of wooden haft or backing that had completely perished without leaving any trace in the form of soil marks.

The discoverer went on to suggest that this hypothetical implement had been a large two-handed saw. This would appear to be quite impossible. The depth of cut of a saw formed by insertion of teeth into a wooden backing would obviously be limited to the length of the exposed portions of the teeth. The average length of the blades found was about  $\frac{1}{2}$  inch, so that if it be assumed that they were inserted to the extent of half their length the possible depth of cut would have been about  $\frac{1}{8}$  inch. To say that it appears unlikely that an implement which (including the necessary handles) would have been about 7 feet long would have been constructed merely for the purpose of making a cut of this depth would appear to be something of an understatement. Even had it been made, however, it would have been of no practical use whatever, since at the first stroke such of the teeth as escaped breakage would inevitably have been dragged from their anchorages by the very heavy longitudinal strain imposed.

'Swords' (more accurately spiked clubs) made by inserting blades—generally sharks' teeth—along the edges of a flat wooden

club are of course familiar museum objects in Oceanic collections, but there appear two objections to the interpretation of the White Hill find on these lines. In the first place such a weapon if of the size indicated by the length of the row of blades would seem too large and clumsy to be serviceable; secondly, there appears to be no independent evidence for the use of a composite weapon of this kind in mesolithic or early neolithic contexts in Europe and in its absence, explanation based on evidence from regions so far removed both in space and time is best eschewed.

It should be emphasized that the existence of the wooden haft or backing in this instance is purely a matter of inference, no actual trace having been observed; however, since the regular linear arrangement of the blades can obviously not be fortuitous, the only alternative explanation is that they were deliberately arranged on the ground in this manner, and such a proceeding would appear so pointless that so long as the discovery remained isolated one would hesitate to accept such an explanation.

However, a parallel to the White Hill discovery appears to be provided by the record, in Dr. Field's paper, of '19 blades of "Solutrian" type' lying 'arranged in a circle 1 metre in diameter' at the site referred to as 'Aramco Camp G2554.' The site would appear to be considered to be pre-neolithic. The analogy with the White Hill find is evident, and it is significant that in the Aramco Camp discovery the large size of the implements as well as their circular arrangement preclude any possibility of their having formed part of a composite tool or weapon. It would not in the circumstances appear to be stretching the bounds of possibility unduly to suppose that whatever motives operated to lead to the symmetrical arrangement noted at Aramco Camp may have been paralleled in connexion with the British find (probably not very dissimilar in age). If this view be accepted the search for an explanation of the latter on "composite implement" lines becomes superfluous.

Hoards of flint or stone implements or of both, though uncommon in comparison with those of metal objects, are of course sufficiently well-known and ordinarily a purely utilitarian explanation appears adequate, but the careful arrangement of items in such a hoard in a symmetrical pattern, having no practical value, is probably to be interpreted as having its basis in some ritual requirement. The White Hill and Aramco finds may, therefore, have been votive deposits.

Bristol

R. D. GREENAWAY

#### The Social Anthropology of India. Cf. MAN, 1956, 119

**169**

SIR.—After *African Political Systems* appeared in 1940, it was reviewed at some length in MAN (1941, 24) by Dr. Meek. The book was, as he recognized, a first and important step and was followed by the remarkable development of African sociology which we observe today.

The reasons for the emphasis upon Africa in the last 25 years are part of the history of the subject, and no less significant is the backwardness of Indian sociology until very recently. The first modern monograph was *Religion and Society among the Coorgs*, by M. N. Srinivas, not published until 1952. However, the kind of profit which Srinivas drew from his Africanist colleagues in that work suggests that Indian sociology so far from suffering might benefit from the results achieved elsewhere while India lay fallow. Srinivas's perception of the dominant caste and its significance could only have come to a worker whose early training involved a study of *African Political Systems*. That he was the first writer to perceive this is an obvious example of the profitable relation between Indian and African studies.

The appearance, then, at this time of *Village India* (reviewed in MAN, 1956, 119) was welcome, for it was the first work by various hands in any way comparable with *African Political Systems*. The ways in which it is like the earlier work and the ways in which it is not should be of interest to any sociologist regardless of his particular area. It lacks pre-eminently that controlling body of assumptions as to method and interests which, despite the individuality of each contributor to *African Political Systems*, makes that book a sociological classic. Furthermore, apart from Professor Srinivas and Dr.

Gough, the contributors to *Village India* are trained in the American tradition and are not concerned to ask those questions of their material which British social anthropologists ask. This in itself is a fact of considerable interest for the future of social anthropology as a whole, integrated subject. One would have hoped that some comparison and evaluation of the various points of view would have been the recognized duty of the reviewer.

If I am correct in ascribing this significance to *Village India*, and I am sure that I am right in welcoming it as the first manual of comparative studies in the subject, it is disappointing that so far from receiving the considered attention which *African Political Systems* received, it should be dismissed in three descriptive paragraphs which tell one no more about the book than might be gathered from a glance at the contents page. A serious work of collaboration which sets out to deal with some of the fundamental problems of Hindu society surely might expect a less cursory treatment. Moreover, without a certain measure of critical interest and sympathy, the relation between African and Indian sociology may well suffer, to the possible detriment of both branches of the one discipline.

D. F. POCOCK

*Institute of Social Anthropology, University of Oxford*

**'Utrilateral' and 'Utrolocal.'** Cf. MAN, 1956, 30, 93

Sir,—Dr. Freeman's letter about his proposed new words 'utrilateral' and 'utrolocal' leaves me unconvinced, for the following reasons:

1. Of 14 anthropology students who were asked to read out 'utrilateral' nearly all had to make a special and obvious effort to enunciate it clearly and six stumbled badly and had to repeat it at least once. Other individuals approached at random on the campus, including anthropologists and other faculty members, had similar difficulties. An experimental psychologist tells me that this is because of the tested difficulty of pronouncing words featuring 'homogeneous chaining,' in this case 'tr...ter,' a feature which deserves the attention of neologists.

2. A number of individuals approached, including a linguist and a social anthropologist, reported a tendency to associate 'utrilateral' with 'uterine,' in spite of elucidation of its etymology and of Dr. Freeman's precaution.

3. Words compounded with 'ambi-' are commonly used to mean 'either,' i.e. both but not necessarily both at the same time. This is denoted by 'bi-' meaning 'twice, doubly, having two; in two ways or directions, on both sides' (O.E.D.). Cf. 'ambiguity—wavering of opinion; word or phrase susceptible of more than one meaning'; 'ambidextrous—able to use both hands alike,' but in common understanding not both at the same time; 'ambivalent—having either, or both, of two contrary values or qualities.' In practically all the 'ambi-' words in the *Oxford English Dictionary* the common element is wavering, doubt, choice—in other words, either, not both in the sense of double or simultaneously associated with each side. In such cases use and connotation are more relevant than literal etymology.

4. Firth introduced 'ambilateral' and applied it to the Maori *hapu*, but this need not be its only reference. More recently, Leach has used the term of descent systems in which at any point in a genealogy kinship may be reckoned through either the father or the mother, in which succession may be through either a male or a female (*Social Science Research in Sarawak*, 1950, p. 62). This is evidently the case with the Iban. The term denotes 'kinship systems in which an individual can claim membership or kinship status in both the father's or the mother's birth group, though not necessarily in both at the same time' (Leach, p. 61). Dr. Freeman's term refers to systems 'in which an individual can possess membership of either his father's or his mother's birth group... but not of both at the same time.' The formal definition of 'ambilateral' thus admits the possibility of rights in both parental groups, while 'utrilateral' explicitly excludes the possibility. However, Leach says of the unilineal descent system as found in Borneo that 'its mode of operation probably differs substantially from one group to another'

(p. 62), and it is surely not advisable to coin a separate term for each mode within the same type of system. The term should say what the descent system is not—viz. double descent or unilineal—rather than try to describe it with complete precision.

5. It is not quite clear what the Iban system is. Can an individual born into his father's birth group really not claim membership or kinship status in his mother's birth group? Could he not join the latter group and acquire the rights of a resident member, claiming these by kinship through his mother? The situation which Dr. Freeman outlines for the Iban seems to be common to the tribes of middle Borneo, but in them the latter course is nevertheless possible. If in fact an Iban has no kin rights in the group of one of his parents then perhaps there is room for a new term, but even this case could more conveniently be subsumed under 'ambilateral' as another 'mode of operation' of this type of descent system.

RODNEY NEEDHAM

*University of British Columbia, Vancouver, B.C.*

**Note**

Dr. Needham's first objection is perhaps a little insubstantial; other explanations, in part psychological, could be suggested for difficulties observed in pronouncing such a neologism at first sight. From the point of view of euphony alone, the form 'utrilateral' would probably trip more lightly off the tongue and it is perhaps a pity if it must be avoided in deference to those who see wombs where none are. But the real question at issue between Dr. Needham and Dr. Freeman would seem to be: what degree of precision is desirable in distinguishing by name the institution concerned?—ED.

**'Slash and Burn.'** Cf. MAN, 1955, 144; 1956, 22, 59

MONSIEUR,—Veuillez donner ma voix à M. Bushnell contre l'abominable *Brandwirtschaft*. *Wirtschaft* est à traduire par économie, et si le terme composé pouvait avoir une signification, il devrait se traduire économie incendiaire, ou économie par le feu, sans référence aucune à l'agriculture. Il s'agit en réalité d'un monstre qui, sous l'apparence débonnaire d'un mot normalement formé, appartient à la triste famille des Benelux, Motel, Euratom..., abusive et équivoque contraction de *Brandlandwirtschaft*. Ce dernier terme, non seulement est peu maniable, mais est absurde puisqu'il signifie agriculture incendiaire ou agriculture par le feu, comme s'il pouvait être question d'agriculture avant l'extinction d'un feu en réalité préalable à l'agriculture proprement dite, qui ne le suit pas nécessairement.

*Écobuage*, dit Littré, désigne une 'opération qui consiste à enlever la couche superficielle du terrain et à brûler sur place les matières organiques qu'elle renferme.' Si cette définition était bonne, le terme ne pourrait pas avantageusement remplacer 'slash and burn'. Mais il faut remarquer qu'il n'a jamais été question d'écobuage sur un sol nu, et que l'enlèvement de la couche superficielle du terrain n'était que la méthode la plus simple pour couper à ras une végétation vivante généralement basse, afin de dénuder un terrain et de le rendre exploitable pour l'agriculture après incendie. Littré donne donc du terme une définition abusivement restrictive en se référant à une technique particulière d'écobuage dans les conditions particulières de ses sources d'information. Indépendamment de ce cas particulier, on peut parler d'écobuage chaque fois qu'une association végétale de dimensions quelconques est détruite par le feu en vue de l'agriculture.

Quoique pleinement approprié, le terme a l'inconvénient d'être peu en usage, et de n'être sans doute compréhensible que pour le lecteur de langue française; c'est pourquoi, dans des manuscrits sur des Indiens d'Amérique du Sud à publier un jour, j'ai préféré parler d'agriculture sur brûlis. Il s'agit, dit Littré, 1° d'un terme d'eaux et forêts: partie de forêt incendiée; 2° d'un terme d'agriculture: action de brûler ce qui est à la surface d'un champ pour le fertiliser et le débarrasser. Brûlis désigne donc parfaitement ce dont il s'agit; est-il utilisable pour des composés tels que *Brûlis-Landbau* en allemand, ou *brûlis-farming* en anglais?

*Velluire, Vendée, France*

ERIC DE FAUTEREAU





a. R. Radcliffe-Brown

## ALFRED REGINALD RADCLIFFE-BROWN, F.B.A.,

1881-1955: A MEMOIR\*

by

MEYER FORTES

*William Wyse Professor of Social Anthropology in the University of Cambridge*

**172** Professor A. R. Radcliffe-Brown, Emeritus Professor of Social Anthropology in the University of Oxford and formerly Fellow of All Souls College, died in London on 24 October, 1955, in his seventy-fifth year. He was seriously ill, off and on, throughout the last five years of his life. While at Rhodes University, South Africa, in 1954, he fell and broke some ribs and though he made a remarkable recovery the accident probably aggravated the pulmonary illness which had been sapping his constitution. He never lamented his lot. However ill he was, he always rejoiced to see his friends and to meet younger anthropologists. To the end he remained dedicated to his life-long ideal, the advancement of social anthropology as a scientific and humanistic discipline. The posthumous letter on Australian local organization in the *American Anthropologist* (Vol. LVIII, Part 2 (1956), p. 363) was written in his own hand when he was ill in hospital. It is a fragment, but authentically the work of the master, as clear, pungent and unerring as his earliest controversial articles.

When I last saw him, shortly before his death, he spoke with gusto of Cambridge anthropology in the early days, and of his visits to the Havelock Ellises as a boy. Mrs. Ellis, he said, with a flash of the vanity which was, in him, a sympathetic quality, used to accuse him of having swallowed an encyclopædia. I was reminded of my first meeting with him. This occurred in 1931, when he was on his way to Chicago, and stopped over in England to fulfil his duties as President of Section H of the British Association for the Advancement of Science, which was meeting in London. Evans-Pritchard was our host. He was recently back from the Nuer, and he soon drew Radcliffe-Brown into a discussion on Nuer social organization. There followed an impromptu commentary on the concept of the lineage, which was my first experience of Radcliffe-Brown's great gift of lucid yet searching exposition.

Radcliffe-Brown twice received the traditional tribute of homage and affection given to a great teacher by his colleagues and pupils. The first was the volume of essays *Social Anthropology of the North American Indian Tribes*, edited by Fred Eggan and presented to him when he left Chicago in 1937. The Introduction by Robert Redfield is, as Eggan says in his preface to the re-issue (1955) of the book, 'still the best brief evaluation of Radcliffe-Brown and his contributions to Anthropology.' The second was the volume of essays entitled *Social Structure: Studies Presented to A. R. Radcliffe-Brown*, which I had the privilege of editing for presentation to him when he retired from the Oxford Chair in 1946. The Preface supplements Redfield's with an outline of Radcliffe-Brown's career.

Further details are given in the Foreword by Evans-Pritchard and Eggan to the collection of Radcliffe-Brown's papers published in 1952 with the title *Structure and Function in Primitive Society*.

Of Warwickshire stock, Radcliffe-Brown was educated at King Edward's High School, Birmingham. After a year of pre-medical science at Birmingham, he gained a scholarship at Trinity College, Cambridge, in 1901, and held it till 1906. He told me that he originally hoped to read for the Natural Sciences Tripos but chose Mental and Moral Science on the advice of the Tutor. At that time Economics and Experimental Psychology were still in the curriculum of this Tripos and Radcliffe-Brown received a good grounding in both. He took his degree in 1904, with a First Class, first division. It was through his work in experimental psychology that he came into close contact with C. S. Myers and W. H. R. Rivers and through the latter with anthropology and with A. C. Haddon. He has recorded that he was Rivers's first pupil in anthropology, in 1904. His name stands first in a chronological list of 'appointments gained by former students' which Haddon included in a privately circulated memorandum on anthropology at Cambridge dated 21 June, 1923. This is what Haddon says: 'A. R. Brown, M.A., former Fellow of Trinity College, has done absolutely first-class work in his studies and publications on the Andamanese and on various Australian tribes. He was elected Anthony Wilkin Student in 1906 and again in 1909. He is now Professor of Social Anthropology in the University of Cape Town.'

Radcliffe-Brown's first direct contact with those parts of the world in which his life work eventually lay was in 1905. He accompanied the British Association for the Advancement of Science to its South African meeting as one of the secretaries of Section H, of which Haddon was the President. The visitors travelled as far north as the Victoria Falls; and it is of interest to note that one of the papers read to Section H was by Junod on the Bathonga.

In 1906 Radcliffe-Brown went to the Andaman Islands as Anthony Wilkin Student in Ethnology. He returned in 1908 and was elected to the Fellowship at Trinity College mentioned in Haddon's note. His Fellowship thesis was the first draft of what later became *The Andaman Islanders*. It was mainly descriptive, including in particular the account of Andamanese technology that is printed as an appendix to the book. At this stage he was still following closely the methods of his teachers, Haddon and Rivers.

He held his Fellowship until 1914. In 1909-10 he held the post of Reader in Ethnology at the London School of Economics, where he lectured on the Australian aborigines and on the potlatch of the North-West Coast American Indians. He also gave a course of lectures on Comparative Sociology at Cambridge. It was at this time, as the Preface

\* With Plate K, after the portrait in the Royal Anthropological Institute

to *The Andaman Islanders* tells us, that he was working out the sociological method of interpreting primitive social institutions exemplified in Chapters V and VI of that book. The syllabus of the Cambridge lectures indicates how his ideas were developing. He had visited France about this time and been in touch with Durkheim and Mauss, and there is an obvious Durkheimian slant in the lectures. The second, for example, was entitled 'The Classification of Social Types' and the eleventh 'The Social Origin of General Ideas.'

Re-elected Anthony Wilkin Student, he went out to Australia in 1910. The next two years were spent in field research. The results were outlined in the 1913 paper in the *Journal of the R.A.I.* This investigation laid the foundations of his profound knowledge of Australian social organization and of his life-long study of kinship systems.

Back in England in 1913, Radcliffe-Brown completed his revision of *The Andaman Islanders*, but owing to the outbreak of the First World War it was not published until 1922. It is worth recording that he gave a course of lectures on Social Anthropology at Birmingham University during December, 1913, and January, 1914. This arose out of the 1913 meeting of the British Association for the Advancement of Science which was held at Birmingham. The President of Section H, Sir Richard Temple, devoted most of his Presidential address to a plea for extending the teaching of anthropology in the universities and proposed that Birmingham University should give the lead. His emphasis was on the utility of anthropological studies for those whose work as administrators, missionaries, or traders would take them to foreign parts and that was why he thought that Birmingham, with its world-wide commercial connexions, was so suitable as a centre for them. The response of the University was to invite Radcliffe-Brown to give this course of lectures. Temple's high opinion of Radcliffe-Brown is best seen in the review he wrote, in 1922 (*MAN*, 1922, 71), of *The Andaman Islanders*, which he enthusiastically hailed as 'a revolutionary theory of social anthropology.'

The lectures were reported in the Birmingham press. They covered much the same ground as the earlier Cambridge course. The note struck is one that became familiar later. In the first lecture Radcliffe-Brown explained what he meant by the scientific study of society, referring for illustration to Utopian politics. It was a mistake, he said, to think that changes in society could be brought about simply by getting a majority of voters to back some legislation. For human society was, to quote the newspaper report, 'just as much the product of natural law as any other sort of phenomena in the universe' and social institutions were 'the result of certain principles which they could by study discover.' A later lecture compares 'savage societies' in which 'the number of persons in effective social contact with one another was limited,' and 'the social structure was very simple,' with complex civilized societies. The concept of social evolution is distinguished from that of social progress, which is not susceptible of scientific solution and must be left to politicians and reformers. Illustrating social evolution by reference to language, he is reported as saying

that 'if an effort were made to try to change some parts of the system of society . . . the whole would fall to pieces . . .' Two lectures deal with 'moral and juridical institutions' and illustrate 'the nature of moral obligation . . . by reference to different forms of the family and clan,' and two are devoted to 'religious institutions.' Radcliffe-Brown had the distinction twice more of delivering special courses of lectures at Birmingham University. He gave the Muirhead lectures in 1946-47 and the Josiah Mason lectures in 1952. The latter summed up his ideas on Australian cosmology and it is to be hoped that any material which he has left on this subject, on which he was an unsurpassed authority, may one day be published.

When the First World War broke out Radcliffe-Brown was in Australia again. The British Association held its meeting for that year in Australia and he was amongst the members attending it. Section H, on that occasion, presented a unique cross-section of British anthropology. A prophetic eye might have discerned the pattern of things to come in its membership and in the papers offered. Elliot Smith spoke on mummification and megaliths as evidence of the diffusion by migrations from the Eastern Mediterranean to the Pacific and even America. Rivers elaborated these ideas in a paper arguing that Australian culture is allied to Melanesian cultures and that both types are the result of the infiltration of immigrant seafaring peoples. He gave a second paper purporting to demonstrate that 'all the forms of marriage which would be the natural result of monopoly of the young women by the old men are thus now known to accompany the gerontocracy of Australia.'

But what is much more revealing from our present point of view is the contrast between the papers given by Radcliffe-Brown and Malinowski. Radcliffe-Brown's paper is formal and analytical. It is a proposal for classifying the varieties of Australian totemism. First there is a tabulation of the different kinds of totemism, defined as a 'special magico-religious relation between an individual or a social group . . . and a class of natural objects. . . .' Thus he distinguishes different forms of clan, local-group, section, sex, personal, etc., totemism. Then he uses this classification as a basis for determining a series of regional types. He lists nine types—Kariera, Burduna, etc. The Kariera type, for instance, has totemic clans with male descent, multiple totems, Talu cult, no prohibition against killing or eating the totem. The other types are different. The analysis thus begins by establishing what are the variables comprised within the institution of totemism and then distinguishes regional types that vary in accordance with the different ways in which these variables are combined. Short and formal as the summary in the *Report of the 84th Meeting of the British Association for the Advancement of Science* is, it clearly foreshadows the method used in 'The Social Organization of Australian Tribes.' Malinowski's paper, on the other hand, is pure theory and polemic. He discusses the distinction between the sacred and the profane put forward in Durkheim's recently published *Elementary Forms of the Religious Life* (1912). Referring to the ethnographical works of Spencer and Gillen, the Seligmans and

Thurnwald, he concludes that the division is not an 'essential and fundamental feature of religion.'

Illness prevented Radcliffe-Brown from returning to England in 1914, and from continuing his fieldwork in Australia. At first he taught in a Sydney grammar school. Later, in 1916, he went to Tonga as Director of Education and stayed there till he was ordered to leave on grounds of health in 1919. It was not an experience that he relished but it was valuable in giving him direct contact with a Polynesian society. It served also to strengthen an interest in applied anthropology, which he turned to good effect in later years.

Personal ties next took him back to South Africa, where he was able to visit Basutoland. Soon after he received an appointment as Ethnologist to the Transvaal Museum in Pretoria. Here he was almost wholly occupied with technology and physical anthropology. As a pupil of Haddon's he was well trained in both of these branches of anthropology, but he was not primarily interested in either.

A great turning point came in 1921. South Africa was changing from a mainly agricultural to a mainly industrial and urban economy. This led to a rapid flow of African labour to industrial areas; and the historical 'Native Problem' thus took a new and more intractable turn. A demand arose, from both political and academic circles, for the dispassionate study of the native peoples of South Africa; and the first step was the establishment, in 1920, of a Chair of Anthropology in the University of Cape Town, as the nucleus of a School of African Life and Languages. We can appreciate this far-sighted act if we remember that there was, at that date, not a single full-time professorship of anthropology in any British university.

Radcliffe-Brown was appointed to the new Chair. It was an appointment widely welcomed. His Inaugural Lecture, given in August, 1921, was discussed in leading articles in all the important South African newspapers. 'All who are interested in the native question,' said the *Cape Argus* (27 August, 1921), '... will ponder what he has to say on this pressing and ever-present subject.' The theme of the lecture was the emerging social changes due to the mutual relations of White and Bantu, the need for systematic study of 'the native mind' as the newspapers put it, and the way in which theoretical anthropology regarded the issues. He repeated what he had said in a public lecture a year earlier in Johannesburg that 'all the various customs, institutions and beliefs of a society formed together a closely connected system ... change one part of the social system and you inevitably produce a far-spreading movement...' (*Rand Daily Mail*, 8 September, 1920).

Radcliffe-Brown had five years at Cape Town. He quickly became one of the best-known academic personalities of South Africa. His brilliance as a lecturer, his wide learning, his connoisseurship in the arts, his single-minded advocacy of an approach to political and social issues through science and reason, his distinguished bearing, attracted notice both inside and outside the University. He was in great demand as a speaker at academic celebrations (for instance, the Jubilee of Huguenot College at Wellington) and at conferences and public meetings concerned

with educational matters or questions of social policy relating to the Bantu peoples. He used these occasions to drive home the need for the systematic study of Bantu social life by trained students as a basis of enlightened policy. A particularly useful venture was the organization of vacation courses. These were attended by missionaries and administrators from the Reserves and by others concerned with Bantu affairs. Radcliffe-Brown gave the lectures on Social Anthropology and on the Ethnology of Africa and of the Bantu peoples. Other specialists lectured on Bantu history and languages, and on problems of Bantu law, administration and education.

Radcliffe-Brown's introductory lecture to the second course made so great an impression that it was published in full in the *Cape Times* (9 January, 1924). It is worth referring to the examples he gave to illustrate how scientific study bears on practical administration—though he was careful to insist that the application of anthropological knowledge must be left to those whose professional task it is. Why, he asked, had there been a rebellion in Zululand when an attempt was made to introduce a poll tax? It was because this was a direct attack on the 'fundamental principle of family organization, a principle as sacred to the Zulu as any of ours are to us.' This is the principle that every member of a household except the actual head is 'in a position of dependence, of infancy in the legal sense.' To put a poll tax on such a person is to 'admit an immediate relation between him and the State instead of the mediate relation through the father or guardian . . .' He spoke also of the *lobola* custom, and outlined his theory of bride price as the means of establishing paternal rights over the children and as a compensation to the family losing a daughter. Finally he warned against regarding the belief in witchcraft as due merely to ignorance. 'The function of the belief . . . is . . . that it provides an outlet in action for the social passions,' that is, the anxiety and distress aroused by things going wrong.

The respected Bantu savant, Professor D. D. T. Jabavu of Fort Hare, paid an enthusiastic tribute to Radcliffe-Brown's vacation-course lectures in a newspaper article praising especially the 'practical and constructive conclusions' which he drew. In fact, the lectures were popular presentations of theoretical ideas that Radcliffe-Brown was exploring at the time. Some of these appear in the famous papers on 'Methods' (1923) and 'The Mother's Brother' (1924); others, such as the contrast between 'mediate' and 'immediate' relations to the State, we are now rediscovering. *The Andaman Islanders* was published in 1922, but it is not a product of this period. Radcliffe-Brown later regretted that he had not had the time or the funds for extended fieldwork in South Africa. He was, however, able to pay two visits to the Transkeian Territories, where he met and had discussions with the local administrative officers. Writing to him in 1925 to express regret at hearing of his impending departure, the Chief Magistrate of the Territories said: 'I can assure you that you have opened up new avenues of thought and enquiry to us . . .' The letter goes on to speak of the 'deeper and more intelligent interest in the problems which so frequently confront us . . .' aroused

by Radcliffe-Brown's visit, and ends with a hope that he would soon return to South Africa.

Radcliffe-Brown resigned his professorship at Cape Town on being invited to take the new Chair of Social Anthropology at the University of Sydney. South African newspapers expressed the disappointment widely felt. They contrasted the South African Government's niggardly attitude about funds for anthropological teaching and research with the generous provision made by the Australian governments and the Rockefeller Foundation for the Sydney department. Radcliffe-Brown's departure, wrote the *Rand Daily Mail*, 'is an incalculable loss for the country.' It should 'draw attention to the stepmotherly manner in which one of our most valuable sciences is being treated by public and Government alike' said the *Cape Times* (23 November, 1925). There could be no better testimony to the impact Radcliffe-Brown made on South African intellectual life.

Radcliffe-Brown stayed five years at Sydney. His arrival there was welcomed as a landmark in Australian academic history. The reasons are well stated in the following comment in the *Sydney Morning Herald* of 15 June, 1927. 'Australia has owed herself such a chair as Professor Radcliffe-Brown fills not only because of the research work crying out to be done before it is too late among the fast vanishing tribes of our own aborigines, but also because of the increased responsibilities in New Guinea . . .' In this atmosphere of encouragement, and with adequate funds at his disposal, Radcliffe-Brown soon established the nucleus of a research school, in addition to undergraduate courses that became highly popular, and training courses for administrative officers bound for Papua and New Guinea. The journal *Oceania* was founded, and among its first publications was the series of articles that were later re-issued as 'The Social Organization of Australian Tribes.' He returned, in this paper, to his earlier interest in the comparative morphology of Australian social organization, but with a new depth of theoretical insight. A complementary paper on 'The Sociological Theory of Totemism' which he gave at the Fourth Pacific Science Congress in Java in 1930 applies the same method of analysis to the problem sketched in the 1914 paper to the British Association.

In 1931 Radcliffe-Brown went to the University of Chicago. His career there is best described in the words of his closest friends and pupils of that period, Professors Lloyd Warner and Fred Eggan, from whose obituary memoir in the *American Anthropologist* (Vol. LVIII, Part 3, 1956, pp. 544-6) I quote. 'For the first time in many years,' they say, 'he had no administrative burdens and greater leisure to write and teach. He renewed his earlier acquaintance with American Indian social organization, developed his conceptions of primitive law and social sanctions, and laid the framework for the modern treatment of the lineage. He systematized and expanded his conception of social anthropology as the comparative study of society in a brilliant series of lectures and seminars, and encouraged the application of social anthropological methods to Western and Far Eastern societies. He became an important and controversial figure in American anthropology.'

But his stay in Chicago was short. In 1937 he returned to England to take up the newly created chair of Social Anthropology at Oxford. Here he was in his element. His task was not as it had been at Cape Town and Sydney, and to some extent even at Chicago, to establish social anthropology in the University. This had already been done. It was to deploy the scholarly importance and demonstrate the high intellectual quality of his subject. The war played havoc with these aims. Indeed he was away from Oxford, in Brazil, on a cultural mission for the British Council, from 1942 to 1944. Nevertheless he succeeded in making a distinguished place for the study of social anthropology at Oxford, and the influence of the Oxford school was widely felt.

What is sometimes called the 'British school' of post-war social anthropology gained its chief impetus from Radcliffe-Brown during this period. He came to Oxford at a critical moment in British social anthropology. The rich harvest of intensive field research stimulated by Malinowski was coming in fast. It threw into relief theoretical problems of social organization which were Radcliffe-Brown's speciality. His publications in the decade 1940-1950 and his lectures and seminars at Oxford provided the conceptual foundations for dealing with these problems. As a teacher, Radcliffe-Brown excelled. He warmed to an audience, especially if it was young. His exceptional ability to expound difficult problems with ease and clarity and his delight in systematic analysis were most happily expressed in his lectures, or better still in the informal sessions with friends and pupils over drinks which he loved. He had an imposing and world-wide knowledge of ethnography which he kept well up to date, and he kept in close touch with current work in philosophy and sociology too. Always eager to know what younger anthropologists were doing and to argue problems with them, his thinking was always ahead of his infrequent and often short publications. Some of his most fertile ideas were put forward in lectures and conversation and came into circulation through the work of his friends and pupils.

When Radcliffe-Brown retired from Oxford in 1946 he was invited to establish a department of Sociology at the Farouk I University at Alexandria. In 1950 he was elected Simon Visiting Professor at Manchester University, and later went out to Rhodes University, Grahamstown, first as a Research Fellow and then as Visiting Professor. He returned to England in 1954, a very sick man, but with unquenched eagerness to maintain his contacts with social anthropology. An event that will long be remembered was his presence at the meeting of the Association of Social Anthropologists in January, 1955. He rose from his sick bed in a London hospital to preside, and the ovation he received from the gathering, which included a sizable number of young anthropologists who had never met him, touched him deeply. But what was memorable was his superb summing-up of the discussion and the ideas he threw out as he made his comments.

This is not the place to attempt a balance sheet of Radcliffe-Brown's contribution to twentieth-century social anthropology. To have established social anthropology as

a reputable academic study in some of the most famous universities in the English-speaking world, and elsewhere too, would, in itself, be a claim to a place of high honour in the history of anthropological science. But his achievement is far greater. He will go down in history as one of the creators of modern social anthropology and his part in this was revolutionary. That is to say, he developed methods and generalizations which set new tasks and opened new possibilities in social anthropology. Much as he owed to his predecessors, he made additions of such originality to the body of anthropological science that we are still assimilating them. Indeed his ideas and theories are more influential today than ever before. This is due to the fact that they never became rigid. Consistent as his aims, and his general framework of thought, remained right through his career, his specific methods and hypotheses never became inflexible. He improved and modified them with every advance in knowledge. What he strove for, above all, was increasing precision and generality and he reached a very high level in these respects. It has often been remarked that Radcliffe-Brown wrote little in comparison with other leading anthropologists of his generation. It is important to add that everything he wrote is still significant. This is as true of his short notes, controversial articles and reviews as of the better-known and more systematic papers. They are pregnant with ideas and insight which will inspire field research and theory for many years to come.

Radcliffe-Brown's connexion with the Royal Anthropological Institute went back to 1909, when he became a Fellow, and he supported the Institute loyally all his life. He was President in the difficult years 1940–1942 and later served very conscientiously on the Council and on Institute committees. He gave devoted service also to other bodies concerned with promoting anthropological study and research, for example the International African Institute. The Royal Anthropological Institute conferred various honours on him. He was awarded the Rivers Medal in 1938, was elected the first Henry Myers Lecturer in 1945, and received the Huxley Memorial Medal in 1951. He took great pride in these honours and also in his life presidency of the Association of Social Anthropologists. He was elected a Fellow of the British Academy in 1950 and was a Member of the Royal Netherlands Academy of Sciences.

What manner of man was 'R.-B.' as his friends and pupils usually called him? He was tall and of striking

appearance. As a young man at Cambridge he had the nickname of 'Anarchy Brown.' This was a friendly recognition of the streak of aloofness in him and of his reputation for holding somewhat highbrow ideas in matters of art, life and literature. But though he was always somewhat reserved and had intimate ties with only a very few close friends, there was nothing of the recluse or the ivory-tower scholar about him. He greatly enjoyed both giving and receiving hospitality and was as genial in the company of a group of students as in the Common Room of All Souls. He took the greatest pleasure in good food, good wine and, most of all, good conversation. Though he could be severe to the point of scorn with slipshod thought and was repelled by bigotry or irrational prejudices of any kind, he was quite incapable of malice. He was unfailingly helpful to younger students. He read their manuscripts, gave them advice, and made fertile theoretical suggestions for their work, without stinting time or patience. He was gentle with his friends and happy in their affection. For a man so detached in his personal relationships and his philosophy of life, his bonds with them were very close. He admired and was influenced by classical Chinese culture and philosophy and by the writers of the French Enlightenment. He was a poor academic politician for he did not care to seek after power. But he will be remembered by those who knew him well most of all for his single-minded and life-long devotion to the advancement of anthropology and his ability to inspire others with this ideal.

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- 1950 'Introduction' to *African Systems of Kinship and Marriage*, edited by A. R. Radcliffe-Brown and Daryll Forde, Oxford University Press.
- 1951 'The Comparative Method in Social Anthropology' (The Huxley Memorial Lecture for 1951), *J. R. Anthropol. Inst.*, Vol. LXXXI.
- 1951 'Murngin Social Organization,' *Amer. Anthropol.*, N.S., Vol. LIII, No. 1.
- 1952 'Introduction' to *Structure and Function in Primitive Society*, by A. R. Radcliffe-Brown, London (Cohen & West).
- 1956 'On Australian Local Organization,' *Amer. Anthropol.*, N.S., Vol. LVIII, No. 2.

## THE CHRONOLOGY OF THE HARAPPAN CIVILIZATION AND THE ARYAN INVASIONS RECENT ARCHAEOLOGICAL RESEARCH\*

by

WALTER A. FAIRSERVIS, JR.

*American Museum of Natural History, New York*

173 Unfortunately for purposes of immediate reference, our final report on the archaeology of the Quetta Valley is unavailable at this writing. The report

\* With a table

has, however, been completed, and is at present in the press.<sup>1</sup> The data for that area which are used herein are fully described in that report.

The sequence of prehistoric cultures in the Quetta

Valley has been defined by excavations. It is as follows:

Damb Sadaat III
Damb Sadaat II
Damb Sadaat I
KGM (Kili Ghul Mohammad) IV
KGM (Kili Ghul Mohammad) III
KGM (Kili Ghul Mohammad) II
KGM (Kili Ghul Mohammad) I

KGM I is the preceramic horizon outlined in my preliminary report.<sup>2</sup> (I have abandoned the term 'Kili' used for these levels previously and now use the more appropriate abbreviation KGM to describe the sequence at that site of Kili Ghul Mohammad). The KGM II levels contain rather coarse hand-made pottery, much of which is basket-impressed. Occasional painted decoration occurs. KGM III is marked by the appearance of the first wheel-made wares. The decorative style features black on red slip and simple designs which strongly suggest northern Iran as their place of origin. The succeeding KGM IV levels contain the so-called Kechi Beg wares which have a strong suggestion of the Amri tradition in their decorative style. Damb Sadaat I appears to be a later phase of the Kechi Beg style with typical Amri-like polychrome decoration added. Damb Sadaat II represents a period when the elaborate decorative style of the so-called Quetta ware was in full vogue. Accompanying this ware is the fine black-on-grey pottery which has a wide distribution in Baluchistan. Damb Sadaat III represents the last known prehistoric culture in the Quetta Valley. It is characterized by a limited repertory of painted pottery designs, *i.e.* the Sadaat, and an elaborate fertility cult which includes bull figurines and 'Mother Goddesses' of the so-called 'Zhab' type.

The entire sequence appears to have continued without significant break from preceramic times and this conclusion appears to be confirmed by the overlapping of pottery styles. It is therefore significant that it is only in the Damb Sadaat III levels that contact with the Harappan civilization occurs. Two carbon-14 samples for the Damb Sadaat II levels were tested and exactly the same date for each was arrived at, *viz.* c. 2100 B.C. ( $4050 \pm 200$  years;  $4050 \pm 400$  years). If one accepts this date the Damb Sadaat III levels represent a period after 2000 B.C. Since these levels are rather extensive (12 feet deep at Damb Sadaat—at least two building phases represented) it may well be that Damb Sadaat III represents a considerable period of time, somewhere let us say between 2000 and 1500 B.C. In the Bolan Pass a Harappan-Mehi site was found which indicates an extension of the Harappan civilization towards the Quetta Valley. The evidence in the Quetta Valley thus indicates that this extension was taking place after 2000 B.C.

Turning from the Quetta Valley to Loralai, the district of northern Baluchistan east of the Quetta Valley, we discover strong evidence for the lateness of the Harappan contacts in this portion of Baluchistan. This evidence is based on research which is at present under way on the materials brought back from Loralai as the result of survey and excavation there in 1950-1951. Three sites are particularly important in this area, because they are in fairly close

proximity to one another. These sites are Rana Ghundai (the northernmost), Sur Jangal (in the middle), and Dabar Kot (on the south). Our excavations at Sur Jangal<sup>3</sup> (conclusions in the preliminary report now revised) indicate three distinct ceramic traditions at the site, *viz.* Sur Jangal I, II, III.

Sur Jangal I has both a coarse hand-made decorated ware and a wheel-made ware decorated in the style of KGM III in the Quetta Valley. The type is found also in Rana Ghundai I. Sur Jangal II is clearly influenced by the Kechi Beg—Amri tradition as defined in KGM IV—Damb Sadaat I in the Quetta Valley. The fine-line decorative style of the Piggott-Ross Rana Ghundai II type is found in these levels as are a number of sherds of de Cardi's Togau style. Sur Jangal III is characterized by the broad black-on-red painting most typically found at Periano Ghundai in Zhab. Here too is the red-on-red ware identified in Rana Ghundai III *b-c.*<sup>4</sup> More important perhaps are the associations of such ceramic types as the black-on-grey ware of Damb Sadaat II. In addition other pottery types found typically in the Quetta Valley in the Damb Sadaat II-III levels are found in Sur Jangal (*e.g.* Quetta wet ware).

The chronological relationship between Rana Ghundai I, II, III, Sur Jangal I, II, III, and the Quetta Valley sequence is thus apparent (see Table I).

When we examine the Sur Jangal material we discover no indications of contact with the Harappan civilization. Yet within 25 miles of Sur Jangal are found two Harappan sites: Duki mound and Dabar Kot. At Dabar Kot surface collections reveal that deep within the mound there are levels which equate to all three prehistoric ceramic traditions found at Sur Jangal and Rana Ghundai.<sup>5</sup> We can thus conclude that the Harappan occupation of Dabar Kot came after the occupation represented by Sur Jangal III. This, of course, confirms the evidence of the Quetta Valley sequence. We are again justified in arguing the lateness of the Harappan civilization's arrival in Baluchistan.

At Harappa Wheeler's recovery of a 'Zhab' ware underlying the Harappan remains at that site is significant, for it appears to be another clue indicative of the time of the Harappan civilization's arrival in the Punjab. The decorative style of the recovered sherds appears to be that of Sur Jangal II-III—Rana Ghundai II-III tradition. In any case the entire Harappan occupation occurs above this level, indicating the possibility of the existence of that occupation at approximately the same time as that of the one at Dabar Kot or in the period just before, *i.e.* Sur Jangal III times.

In Sind we have two interesting pieces of evidence. The first is the occurrence of the Harappan levels over those of the Amri. We are not certain, of course, of the origin of the Amri tradition. It is certainly widespread in Baluchistan. There appear, however, to be two phases of the Amri design style. The first is the very fine line drawing of hachures, sigmas, diamonds, etc.; the second involves polychromy and a broader brush stroke. In the Quetta Valley the latter stage merges in the Quetta-ware design tradition so that it becomes difficult to separate late Kechi Beg style from early Quetta ware. It is therefore interesting to observe at such sites as Pandi Wahi and Ghazi Shah a number of

TABLE I. COMPARATIVE CHRONOLOGY

Carbon-14 Dates Quetta Sequence	Quetta Valley	Sur Jangal	Loralai District Rana Ghundai	Dabar Kot	Punjab Harappa	Sind Mohenjo-Daro	Sind (General)
				RG V			
				RG IV			
					Harappan levels	Upper levels	Jhukar
	Damb Sadaat III	↑ ?	↑ ? C		Harappan levels	Lower levels	Harappan
2100 B.C.	Damb Sadaat II	SJ III	RG III B		'Zhob' style		
	Damb Sadaat I	SJ II	RG II	A			
	KGM (Kili Ghul Mohammad) IV						
	KGM III	SJ I	RG I				
	KGM II						
3300 B.C. (upper levels)	KGM I						Anri

sherds which illustrate a strong tendency towards the Quetta-ware design tradition.<sup>6</sup> This may be an indication of a late Amri style flourishing prior to the Harappan occupation of that site. The second piece of evidence of interest is that of the existence in the so-called 'lower levels' at Mohenjo-Daro of several examples of a ware type named by us 'Quetta wet ware' which is described in the coming report.<sup>7</sup> This ware is very common in the Damb Sadaat II-III levels in the Quetta Valley.

The archaeological evidence at this date thus indicates that the Harappan civilization was in Sind during the period represented by Damb Sadaat II-III in the Quetta Valley and Sur Jangal—Rana Ghundai III in Loralai, that it was expanding northwards into Baluchistan in Damb Sadaat III times and after the end of the occupation of Sur Jangal. If we accept the carbon-14 dating of Damb Sadaat II we must place this expansion in the period after 2000 B.C. If we consider the evidence at Harappa as in any way indicative, we might include the occupation of that site as a part of the same northward expansion of the Harappan civilization as that of Baluchistan. This would mean that Harappa was occupied after 2000 B.C. Considering the chronology as a whole, it seems that the date of 1500 B.C. for the end of the Harappan civilization is too early. A date nearer 1200 B.C. or later is probably more accurate since it allows for the not inconsiderable expansion and establishment of that civilization in areas remote from its apparent place of origin. (The question might be posed here whether the place of origin might be east and north of Sind. In that case the expansion into Sind would have to be considered as no earlier than Damb Sadaat II times on this present evidence.)

The question of the archaeological identification of the Aryan period is directly pertinent to the problem of the chronology and in this regard there appears to be some new evidence. I have pointed out in the final report on the

Quetta Valley that there was an apparent diminution of population after the last 'prehistoric' occupation. (There were no indications of violence at the close of this occupation, though this is only on the evidence furnished by one excavated site, Damb Sadaat.) The pottery type which appears to succeed in the Quetta Valley is a heavy handmade ware decorated rather coarsely with simple designs sometimes in polychrome. These designs are roughly of two types: simple geometric forms and curvilinear motifs, the latter sometimes having a tendril-like appearance. The ware is frequently decorated with raised bands usually incised diagonally or in crosses. This pottery type appears in Rana Ghundai IV.<sup>8</sup> It is common on the surface of Dabar Kot, Periano Ghundai, and Kaudani.<sup>9</sup> There appears to be an identification of this type with one form of de Cardi's Londo ware.<sup>10</sup> I have called this type Ghul ware after the site where it was first identified in the Quetta Valley. It is a ceramic tradition entirely new to Baluchistan and perhaps is therefore indicative of an invasion of some kind. It appears to me to be a good candidate for consideration as a marker of the period of the Aryan occupation.

Another interesting ware type is found in Northern Baluchistan and it too has relevance to problems relating to the Aryan invasion. For some reason, Ross overlooked a ware type which is not infrequently found at Rana Ghundai. We located several fragments in sites at what can be regarded as a Rana Ghundai IV level.<sup>11</sup> This is a wheel-made ware, decorated in broad lines of black and red running horizontally around the body of the vessels. The rim is frequently decorated with loops and hatching. Various other geometric and curvilinear motifs also occur. This type of decoration suggests the Jhukar style more than slightly. Its stratigraphic relationship to Ghul ware at Rana Ghundai is not clear though there is some indication that it is somewhat earlier. This ware is found at Dabar Kot, Moghul Kala, and Kaudani.<sup>12</sup> If this is really a Jhukar type

ware, and I have no reason at present to suggest otherwise, it perhaps indicates the route along which the Aryan invasions came. The Gomel Pass has perhaps been underrated as a major route out of Afghanistan by archaeologists. It certainly was not underrated by invaders of India in the historic past or by the British in the days of the Indian Empire. It is one of the major passes used by the seasonal nomads in their annual migrations today. The evidence for a Jhukar occupation of Baluchistan in the Gomel Pass area suggests that that pass was used by invaders at the end of 'prehistoric' times. It is conceivable that the Jhukar people pushed by militant pressures in Eastern and Southern Afghanistan moved through the Gomel Pass to the Derajat or to the Zhob Valley to Loralai and thus to Sind. The Ghul ware also follows this distribution in general. Therefore it may well be that the Gomel Pass was the critical pass when the invasions of the Indus Valley took place.

It is significant that Rana Ghundai V, the upper levels at Dabar Kot, and Moghul Kala all are featured by the stamped, moulded, *appliqué*, and rope wares common to the Buddhist periods of northern Pakistan and Eastern Afghanistan. If we accept the evidence for a later date for the Aryan invasions and if such ware types as Londo, Ghul, and Jhukar are indicative of the period subsequent to those invasions, it appears that we are making progress towards clarifying the chronology of the post-Harappan Indus Valley and Baluchistan.

#### Notes

<sup>1</sup> W. A. Fairservis, Jr., 'Archaeology of the Quetta Valley, West

Pakistan,' *Anthrop. Papers of A.M.N.H.*, Vol. XLVI, Part 1, 1956 (in press).

<sup>2</sup> *Idem*, 'Preliminary Report on the Prehistoric Archaeology of the Afghan-Baluchi Areas,' *American Museum Novitates*, No. 1587, 12 September, 1952, pp. 9-17.

<sup>3</sup> *Ibid.*, pp. 18-21.

<sup>4</sup> E. J. Ross, 'A Chalcolithic Site in Northern Baluchistan,' *J. Near East. Stud.*, Vol. V, No. 4, October, 1946, p. 305; Plate X, 6, 7; fig. 4, Nos. 3, 4, 6, 11; Stuart Piggott, *Prehistoric India*, London (Pelican Books), 1950, p. 122.

<sup>5</sup> This statement is made on the basis of the surface collections made in 1950 at Dabar Kot. Stein, *Mems. Arch. Surv. Ind.*, No. 37, Delhi, 1929, Plate XIV, D. 15, D. 19, D. 59, Plate XV, D.N. c. 2, illustrates sherds typical of Sur Jangal—Rana Ghundai III; only D. 19 may be of Sur Jangal—Rana Ghundai II levels.

<sup>6</sup> N. G. Majumdar, 'Explorations in Sind,' *Mems. Arch. Surv. Ind.*, No. 48, 1934; Plate XXVII, 23, 49, 50, 51; Plate XXVIII, 8, 24, 30. It is of interest that the decorative style of the sherds illustrated in Plate XXVI, 32, Plate XXVIII, 43, 44, and possibly 38, is in the so-called Faiz Mohammad or Black-on-grey tradition found in Damb Sadaat II. The designs on Plate XXVII, 25, 35, 36, are very typical of Damb Sadaat III. On this basis it would not be surprising to find the Harappan occupation of Sind later than Damb Sadaat III. It is certain that there are other traditions than the Amri (as at present defined) underlying the Harappan levels at these sites.

<sup>7</sup> E. J. H. MacKay, *Further Excavations at Mohenjo-Daro*, Delhi, 1937, Vol. II, Plate LXVII, 1, 2.

<sup>8</sup> Ross, *loc. cit.*, Plate XI, 2, and fig. 5.

<sup>9</sup> Stein, *loc. cit.*, 1929, illustrates one sherd for Dabar Kot, Plate XIV, D. 23; Periano Ghundai, none illustrated; Kaudani, possibly Plate X, K. 2, K. 3, K. 5.

<sup>10</sup> B. de Cardi, 'A New Prehistoric Ware from Baluchistan,' *Iraq*, Vol. XIII, Part 2, 1951, pp. 66 f., fig. 1, No. 6; fig. 2, Nos. 1, 5.

<sup>11</sup> Stein, *loc. cit.*, 1929; Plate XIII does not illustrate this type for Rana Ghundai either.

<sup>12</sup> *Ibid.*, Plate XV, D. E. ii, 6, D. E. ii, 4 (Dabar Kot); Plate XIII M. K. 5, possibly M. K. 8 (Moghul Kala); Stein does not illustrate the type for Kaudani.

## SHORTER NOTES

**The Centenary of Neandertal Man: Part I. By Bernard Campbell, Duckworth Laboratory of Physical Anthropology, University Museum of Archaeology and Ethnology, Cambridge. With a text figure**

**174** This year is the centenary of the discovery of the celebrated prehistoric human fossil from the Neander valley<sup>1</sup> in Germany. An international symposium has recently been held (in August) to discuss problems relating to the fossil, and it therefore seems an appropriate time to recall the history of the discovery and the part it has played in biological thought during the last hundred years.

The skull, which consists merely of the roof of the brain case (calotte), together with a number of limb bones, was discovered by workmen who were clearing out the floor deposits of a cave 60 feet up in the steep limestone cliffs of the Neander valley, above the river Düssel near Hochdal, between Elberfeld and Düsseldorf. The cavern contained a deposit of five feet of loamy mud, and from the base of this the bones were collected. The workmen believed them to be those of an animal and unfortunately collected only the most perfect: it was a local paleontologist, Dr. C. Fuhrmann, who recognized them to be human and saved them for posterity. The mud deposit contained no data of an archaeological or paleontological kind. Sir C. Lyell's sketch of the cave (*The Geological Evidence of the Antiquity of Man*, London, 1863, p. 76, fig. 1) makes it clear that the loamy mud (b), which is quite similar to the surface soil on the plateau above the cave (d), could have been washed down the funnel, with the bones perhaps, at any

time. There was no stalagmitic crust overlying the bones, neither were animal bones or flint implements found in the deposit. Three years later, the canine tooth of a bear having no certain relationship to the skull was found in another branch of the cave. It was not identified.

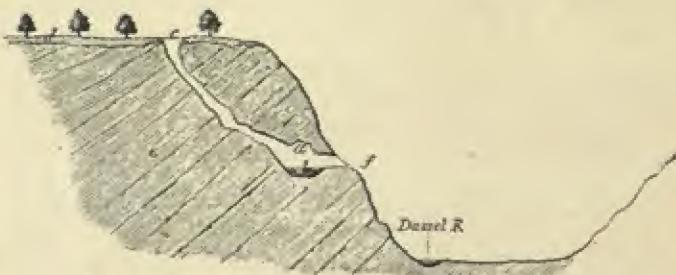


FIG. 1. SECTION OF THE NEANDERTHAL CAVE NEAR DÜSSELDORF

(a) Cavern 60 feet above the Düssel, and 100 feet below the surface of the country at e; (b) Loam covering the floor of the cave near the bottom of which the human skeleton was found; (b, c) Rent connecting the cave with the upper surface of the country; (d) Superficial sandy loam; (e) Devonian limestone; (f) Terrace, or ledge of rock

(After Lyell)

The announcement of the discovery of the calotte came at a time (1857)<sup>2</sup> when the existence of antediluvian man was being bravely supported by Boucher de Perthes and hotly disputed by antiquarians throughout Europe. The discovery, however, had little effect in the dispute, owing to the lack of any recognizable archaeological context. Prehistorians, in fact, overlooked the news almost entirely: their attention was instead to be directed to the excavation of the Brixham cave near Torquay, which in 1858 did so much to tip the balance towards acceptance of antediluvian man.

Fuhlrott handed the skull and bones to Dr. Schaafhausen, who was then Professor of Anatomy at Bonn University, and in 1857<sup>3</sup> Schaafhausen published his first report on them, in which he reached the following conclusions: first, that the calotte belonged to a normal (*i.e.* non-pathological) but hitherto unknown form of humanity; secondly, that it was very ancient, antecedent to the Celts and Germans; thirdly, that it was contemporary with the latest animals of the Diluvium (Pleistocene). Schaafhausen maintained this extreme age in spite of the absence of any proof. Professor F. Mayer of Bonn pointed out at the same time that the dendritic deposits on the bones were no sure indication of great age, as bones that had been quite recently interred often bore similar deposits. He later expressed the view that the calotte was that of a Russian Cossack suffering from rickets who had crawled into the cave to die during the campaign of 1814!<sup>4</sup> In a fuller description the following year,<sup>4</sup> Schaafhausen put forward his views on the significance of the skull, and, in spite of some minor anatomical inaccuracies, he came much nearer to the truth than Mayer, when he concluded that 'there is no reason whatever for regarding the unusual development of the frontal sinuses . . . as an individual or pathological deformity; it is unquestionably a race character, and is physiologically connected with the uncommon thickness of the other bones of the skeleton, which exceeds by about one half the usual proportion.'<sup>5</sup> He also drew attention to what he inferred must be the very low facial angle of the specimen, but noted that the cranial capacity could not have been lower than the average for *Homo sapiens*. As early as 1853, Schaafhausen had published his own theory of organic transmutation,<sup>6</sup> and had written that 'living plants and animals are not separated from the extinct by new creations, but are to be regarded as their descendants through continued reproduction.' The fact that Schaafhausen held such ideas must have considerably aided his acceptance of the Neandertal fossil as a primitive kind of man. Darwin and Wallace's famous paper introducing their theory of evolution and natural selection was read before the Linnean Society during the same year (1858),<sup>7</sup> and no doubt considerably influenced the way in which the discovery was received by British biologists. The fossil was mentioned by one or two journals in England, such as the *Westminster Review*, but the first scientific treatment of the subject was by George Busk, at the time Hunterian Professor of Anatomy and Physiology at the Royal College of Surgeons of London. Busk was a good German scholar and had already published a number of German scientific papers in English. His versatile mind immediately saw the importance of the Neandertal discovery, and he published a translation of Schaafhausen's paper in 1861,<sup>8</sup> together with some remarks and illustrations. Busk deserves praise for bringing the details of the discovery before English-speaking men of science. But he was in a stronger position than Schaafhausen had been; the excavations at Brixham and Arct-sur-Cure, backed by the combined weight of Prestwich and Evans, had served to confirm Perthes's earlier hypothesis. The geological antiquity of man being thus established, Busk was able to remark that the point of interest was to determine 'in what respects the priscan race or races may have differed from those which at present inhabit the earth.' Busk agreed in

general with the views expressed by Schaafhausen, though he disagreed with his conclusion that the superciliary ridge was due to enlarged frontal sinuses. Busk showed that no correlation occurred in other groups between such a large ridge and the extension of the sinuses. In this character he recognized that the skull approached to a remarkable extent the condition found in the chimpanzee and gorilla, and he illustrated the Neandertal and chimpanzee skulls side by side.

The following September (1862), a short paper by Blake, the English anatomist, appeared in *The Geologist*.<sup>9</sup> In this he expressed the view that the skull could not be considered to fall beyond the range of *Homo sapiens*, and a year later he amplified his opinion, concluding that the skull belonged to 'some poor idiot or hermit'.<sup>10</sup> A few months earlier, Thomas Henry Huxley, in a lecture at the Royal Institution, had already maintained that the skulls from Engis and Neandertal were little different in configuration and development from those of Australian aborigines. All these views, however, were based on rather inadequate descriptions and illustrations published by Schaafhausen, and a rather more lively appreciation of the fossil was only made possible by the energies of Sir Charles Lyell, who was at the time preparing his well-known book *The Antiquity of Man*. He travelled in person to Neandertal and there met Fuhlrott, who presented him with a cast of the calotte, and gave him a detailed description of the discovery. On his return home, Lyell gave the cast to Huxley with a request to write a report on it for his book. This Huxley did and in the process he obtained some important photographs from Fuhlrott of the original, which enabled him to present a more complete and valuable account of the fossil than it had previously been possible for any British biologist to do. In 1863 in *The Antiquity of Man*,<sup>11</sup> Lyell included Huxley's report, and Huxley amplified his conclusions and published them in his *Man's Place in Nature*.<sup>12</sup>

Huxley was the first to make a study of the cranial features of the fossil other than the superciliary ridge, the cubic capacity, and one or two general characters of shape: he was first to recognize the low vault and flattened occipital. As a result of his study, he concluded that the fossil was the most 'pithecoid' of human crania yet discovered but, because of its large cranial capacity, could not in any way be intermediate between man and apes, but instead represented a man with strongly developed primitive characters, not very dissimilar from certain prehistoric skulls already discovered at Borreby in Denmark.<sup>13</sup>

The next important paper on the subject was read by William King, Professor of Geology at Queen's College, Galway, at the meeting of the British Association at Newcastle-on-Tyne during September, 1863. King suggested that the Neandertal Cave had received its infilling during the last part of the glacial period on the grounds of its similarity to certain caves in the Meuse valley studied by Lyell. He carried the analysis of the shape of the cranial bones even beyond Huxley, and was so struck by the peculiarity of the skull that the conclusion of his paper is reported as follows:<sup>14</sup>

The author is led to regard the Neanderthal skull as belonging to a creature cranially and psychically different from man; and he proposes to distinguish the species by the name of *Homo Neanderthalensis*.

Since this short report was not accompanied by any reference or description which could give the name validity according to the International Rules of Zoological Nomenclature, the name is not valid as from this publication, but from an amplified paper which appeared in January, 1864.<sup>15</sup> The novelty of this view was considerable; no anatomist had previously suggested a specific distinction for the fossil: Mayer and Blake thought it pathological, as did Virchow shortly afterwards; Schaafhausen, Pruner-Bey

and Broca thought it an ancient Celt or early European of some kind; Huxley and Busk supported such a contention, though emphasizing its pithecid character. Darwin steered clear of the controversy. It was left to King, in 1864, to publish his description of a new species of man, and this paper<sup>15</sup> must receive our recognition for its priority in the nomenclature of the Neandertal species.

No history of Neandertal man would be complete without some mention of the famous Gibraltar skull which was the first human fossil ever discovered which is now acknowledged to fall outside the range of the species *Homo sapiens*. The facts of its discovery are extremely uncertain and the first record of its existence comes from the minutes of the Gibraltar Scientific Society for 3 March, 1848. Here is written 'Presented a Human Skull from Forbes Quarry, North Front, by the Secretary.' It appears that the Secretary at the time was a Lieut. Flint, R.A., to whom we owe the preservation of this valuable specimen: the Forbes Quarry has been identified and examined by Duckworth.<sup>16</sup> Nothing is known of what happened to the skull for the next sixteen years, until, in 1864, it was sent together with some other fossil specimens to Busk and Falconer by a Captain Brome, who was governor of the military prison on the Rock. Brome had developed a keen interest in paleontological discovery since his excavation of the Genista Cave. Encouraged by Busk and Falconer Brome had employed his prisoners in paleontological excavation, and sent numerous fossil bones to England. In a second shipment sent by him, which arrived towards the middle of 1864, he included the human cranium. Duckworth has written<sup>16</sup> that Busk brought the skull to England himself in 1862, and this information was repeated on various occasions by Keith (in his books of 1915 and 1925), but in view of the account that Busk himself wrote of how he received the fossil in London,<sup>17</sup> such a possibility seems very unlikely! It is stated by Keith<sup>18</sup> that, ironically enough, Brome was dismissed from his post and the Service for his employment of prison labour in the cause of science, but for a time at least, it appears that the Secretary of State for War looked in kindly fashion upon his endeavours.<sup>19</sup>

Busk reported the arrival of the skull and described its condition<sup>20</sup>: he realized immediately its resemblance to the Neandertal calotte. Shortly afterwards, while engaged on a paper which they were preparing for the British Association meeting at Bath to be held in September, Falconer wrote to Busk and proposed the name *Homo calpicus*, from the ancient name for Gibraltar.<sup>21</sup> Discussing in this letter their proposed exhibition of the fossil Falconer humorously suggested the following advertisement:

Walk up! Ladies and Gentlemen. Walk up! and see Professor Busk's Grand, Priscan, Pithecid, Mesocephalous, (!) Prognathous, Agriblemmatous, Platynemic, wild *Homo calpicus* of Gibraltar.

No record exists in the report of the name having been mentioned, and it is therefore not valid, even if it had priority! Before the end of the meeting, Busk and Falconer received a grant of £150 from the British Association, and sailed for Gibraltar, to continue the exploration of the Genista Cave. Falconer died soon after, in January, 1865, but not before he had recorded an appreciation of that unfortunate soldier Captain Brome. Our debt to Falconer, as a pioneer in paleontology, is infinitely greater. For his views on the Gibraltar skull, we must turn to some correspondence of the previous year, in which he wrote<sup>20</sup>:

You may see from me that I do not regard this *priscan pithecid man* as the 'missing link' so to speak. It is a case of a very low type of humanity—very low and savage, and of extreme antiquity—but still man, and not a halfway step between man and monkey.

### Notes

<sup>1</sup> The trivial name of the species *Homo neanderthalensis* retains the *h* in *thal* since it predates the revision of German orthography of 1901. In the vernacular name the *h* is dropped according to the modern German usage.

<sup>2</sup> H. Schaafhausen, *Verh. Naturh. Ver. Preuss. Rheinl.*, Bonn, Vol. XIV (1857), pp. xxxviii-xlii, and *Correspondenzblatt* Nr. 2, pp. 50-52.

<sup>3</sup> F. Mayer, 'Ueber die fossilen Ueberreste eines menschlichen Schädels und Skeletes in einer Pelsenhöhle des Düssel- oder Neanderthalen,' *Arch. Anat. Physiol.*, Leipzig, 1864, pp. 1-26.

<sup>4</sup> H. Schaafhausen, 'Zur Kenntnis der ältesten Rassenschädel,' *Müller's Arch.*, 1858, pp. 453-78.

<sup>5</sup> From translation by G. Busk. See note 8.

<sup>6</sup> H. Schaafhausen, 'Ueber Beständigkeit und Umwandlung der Arten,' *Verh. Naturh. Ver. Preuss. Rheinl.*, Bonn, Vol. X (1853), pp. 420-51.

<sup>7</sup> C. Darwin and A. R. Wallace, 'On the Tendency of Species to form Varieties; and on the Perpetuation of Varieties and Species by Natural Means of Selection,' *Proc. Linn. Soc.*, Vol. III (1859), pp. 45-62.

<sup>8</sup> G. Busk, 'On the Crania of the Most Ancient Races of Man. By Professor H. Schaafhausen of Bonn,' *Nat. Hist. Rev.*, N.S., Vol. I (1861), pp. 155-76.

<sup>9</sup> C. C. Blake, 'On the Occurrence of Human Remains Contemporaneous with those of Extinct Animals,' *The Geologist*, Vol. IV (1861), pp. 395-8.

<sup>10</sup> Id., 'On the Cranium of the Most Ancient Races of Men,' *ibid.*, Vol. V (1862), pp. 206-33.

<sup>11</sup> Lyell, *op. cit.*

<sup>12</sup> T. H. Huxley, *Evidence as to Man's Place in Nature*, London, 1863.

<sup>13</sup> Lyell, *op. cit.* See p. 86.

<sup>14</sup> See *Rep. Brit. Ass.* (Newcastle), 1863.

<sup>15</sup> W. King, 'The Reputed Fossil Man of Neanderthal,' *Quart. J. Sci.*, Vol. I (1864), pp. 88-97.

<sup>16</sup> W. L. H. Duckworth, 'Cave Exploration at Gibraltar in September, 1910,' *J. R. Anthropol. Inst.*, Vol. XLI (1911), pp. 350-80.

<sup>17</sup> G. Busk, 'Pithecid Priscan Man from Gibraltar,' *The Reader*, Vol. IV (1864), pp. 109f.

<sup>18</sup> Sir A. Keith, 'The Early History of the Gibraltar Cranium,' *Nature, Lond.*, Vol. LXXXVII (1911), pp. 313f.

<sup>19</sup> H. Falconer, *Paleontolog. Memoirs and Notes*, London, 1868, Vol. II.

<sup>20</sup> Falconer, *op. cit.*, p. 561, footnote.

### The First International Congress of Human Genetics, Copenhagen, 1956. By Dr. D. F. Roberts, Department of Human Anatomy, University Museum, Oxford

An event of considerable significance for physical anthropology was the success of the First International Congress of Human Genetics, held in Copenhagen from 1 to 6 August, 1956. Whereas in some anthropological circles the study of human populations from the genetical point of view is still today regarded as a novelty, the Congress demonstrated how real were the achievements of anthropological studies and how great was their contribution to the more general science of human genetics.

There were nearly 500 participants, representing 26 different countries. Some 225 papers were submitted of which 21 per cent. emanated from the United States, 18 per cent. from Britain and 9 per cent. from Denmark. Of the seven sections of the Congress, that devoted to physical anthropology under the title of 'Population genetics, anthropology and blood groups' was the most vigorous, for here were presented nearly one-third of all communications, Britain with 25 per cent. and the United States with 20 per cent. again being the main contributors. Moreover, a large proportion of the papers read in the plenary sessions and in the remaining sections (concerning internal medicine, psychology, neurology, cytology, ophthalmology, and genetical analysis) were of marked anthropological interest.

The particular attraction of some fields of research was evidenced by the number of authors who spoke on them and of the audience who attended. Such topics included for instance the existing load of human mutations and the possible effects of radiation, the blood groups and their associations with different diseases, the abnormal haemoglobins, the genetic structure of human populations, and the genetic significance of cousin marriages. In the sessions of more specialist appeal several spectacular communications attracted wide interest; for instance the demonstration that man possesses only 46 chromosomes instead of 48, the accepted number for many years. Among the mass of high-quality work presented it is not possible to select for mention here individual contributions; these are shortly to be published in a volume of proceedings.

The success of the Congress was in large measure due to the efficiency with which it was run. The President, Professor Tage

Kemp, and his organizing committee are to be complimented on the result of their efforts.

#### A Biography of L. H. Morgan

**176** Professor Leslie A. White, of the Department of Anthropology, University of Michigan, Ann Arbor, Michigan, is collecting material for a biography of Lewis Henry Morgan (1818-81), and is anxious to locate letters written by him with a view to obtaining photostatic copies where possible. He corresponded with the following to a greater or less extent during the years indicated: Charles Darwin, 1871-77; Sir John Lubbock, 1871; Sir Henry Sumner Maine, 1871-80; J. F. McLennan, dates not certain; Herbert Spencer, 1872-77; Edward Burnett Tylor, 1873; Charles Staniland Wake, 1875. The co-operation of readers who may know of any such letters is invited.

## REVIEWS

### GENERAL

#### Marriage, Authority, and Final Causes: A Study of Unilateral

**177** *Cross-Cousin Marriage*. By George C. Homans and David M. Schneider. Glencoe, Illinois (The Free Press), 1955. Pp. 64. Price \$2

This is a short study of a limited subject, but it has many implications. The authors set out to test the conditions under which unilateral cross-cousin marriage occurs by means of a cross-cultural survey, both in order to check a hypothesis of Professor Lévi-Strauss's and in order to put forward what they consider to be a more satisfactory hypothesis. Lévi-Strauss's hypothesis in his *Les structures élémentaires de la parenté* was that matrilateral cross-cousin marriage for a man occurs more frequently than patrilateral cross-cousin marriage because it leads to 'generalized exchange' of women, the most valuable scarce goods, and hence produces greater solidarity in the society. The present authors considered this to be a 'final cause' theory, and therefore inherently unsatisfactory; and they sought for an 'efficient cause' theory.

In practice unilateral cross-cousin marriage (with marriage to the other cross-cousin disapproved) occurs very rarely. The theory with which the authors started was that the matrilateral form would be found in societies possessing patrilineal kin groups, while the patrilateral form would be found in societies possessing matrilineal kin groups. This is a special hypothesis, arising from a more general theory put forward by Professor Homans in his *The Human Group*: that a man seeks for his 'affectionate' relationships where 'authority' does not lie.

The proposition above was tested on societies in the Yale cross-cultural sample, and a perfect correlation emerged. Feeling that this was too good to be true, the authors hunted out every society they could find in available libraries. The perfection of the hypothesis was disturbed. But after surveying the exceptions, they were able to reformulate the hypothesis to cover every case except one doubtful tribe. The new formulation stated that, in those societies practising unilateral cross-cousin marriage (33 in all), where jural authority over ego male lay with father or father's lineage he would be approved to marry his mother's brother's daughter, and where jural authority lay with mother's brother or mother's brother's lineage, he would be approved to marry father's sister's daughter. It was found that the patrilineal-matrilateral cross-cousin societies were clearly demarcated: special analysis had to be made of matrilineal societies where before marriage a man came under his father's control. The new formulation obviously sorts better with the general Homans hypothesis.

The authors are well aware of the deficiencies of data on the societies considered, and realize that they are working with approved forms of marriage and not actual marriages. For this they are not responsible, and criticism on this point is unfair, though they would welcome corrections. For instance, they take over Dr. Richards's acceptance of the later view of the Ila, published by Dr. Edwin

Smith, that the Ila are not matrilineal, but have double descent. I consider that Dr. Smith has read Professor Forde's analysis of Yakö into the Ila, and it is more likely that Professor Colson's analysis of the neighbouring Tonga applies: the Ila probably recognize as important mother's matrilineal and father's matrilineal groups—not father's patrilineal.

On the basis of the data available, I consider that the authors demonstrate not only that Professor Lévi-Strauss's theory is unsatisfactorily formulated, but also that it is contradicted by the numerical facts. They demonstrate their own correlation even if, as they say, their reasons are wrong. But the reasons seem valid to me; and I hope they will pursue the other problems which they indicate to arise from their study: the extension of their correlation to other forms of marriage; and the investigation of the circumstances in which various forms of preferred and disapproved marriage occur. This is a line of research which is not often followed in Britain; and it is important as a check on the monographic analyses in which we specialize. Studies of this kind also emphasize for fieldworkers new ranges of facts on which they must write explicitly.

This is an excellent study. It raises and solves problems, and it provides a precise model in methodology which will be invaluable for teaching and for research on other problems.

MAX GLUCKMAN

**178** *The Acceptance of Histories*. By Kenneth Bock. Berkeley (U. of Calif. P.), 1956. Pp. 132. Price \$1.75

The purpose of this scholarly essay is to impress upon social scientists the necessity of studying history, and most of it is devoted to showing why they have generally ignored it.

The author begins with Comte, who thought that the data used by historians were not susceptible of scientific treatment; in consequence he preferred the comparative method, which involved regarding societies as organisms. 'Comte and his contemporaries had no real doubts . . . that change was natural, inevitable, progressive, parallel among distinct societies, from the simple to the complex, and, more specifically, from a condition unlike that of modern Western Europe to a condition like it.'

Herbert Spencer followed Comte in regarding societies as organisms, and thought that 'a principal task of sociology was to derive the history of society from an analysis of the nature of society.' Such events as invasions were 'intrusions' in the 'normal processes' of social growth, and were to be ignored.

Tylor followed Comte and Spencer, and sought evidence for the history of culture among survivals in the present, ignoring the records of the past. He trusted to the 'general knowledge' and 'long experience' of ethnologists for support of the proposition that an arrangement of cultures in a progressive sequence probably represented 'their actual sequence in history.' Other leading anthropologists, British and American, of the later nineteenth century are

cited to the same effect, and the author concludes that though their aim was a science of man everywhere and at all times, they refrained from using the historical data of any period, and accepted instead an elaborate philosophy of culture history that provided *a priori* answers to the questions posed by the 'complication of events' in human experience.

He traces the origins of this philosophy to Thucydides and Aristotle, and from them through the schoolmen to the eighteenth-century philosophers, and goes on to show that although the anthropologists of today claim to have discarded this philosophy, with its 'nineteenth-century evolutionism,' they are still inspired by it. Thus Professor Childe chooses to regard as 'vagaries and fluctuations' an appreciable segment of human experience, and for no apparent reason other than the fact that it does not accord with the theory of progress which he has adopted.

Goldenweiser equates the early with the primitive to the extent of regarding the classical Greeks as 'more recent' than the Polynesians.

Professor Evans-Pritchard 'insists that a knowledge of the history of an institution cannot tell us how it works or functions in social life.' 'Working' and 'functioning' of institutions are here regarded as somehow not historical phenomena. The specious distinction was more plainly made when Radcliffe-Brown called for a strict separation between 'a *historical* interest in human life' and a 'scientific understanding of the *nature* of culture and of social life'; and again when Lowie saw as an alternative to an interpretation of 'unique series of events' the formulation of 'general sociological principles . . . independent of the time factor.' The author asks whether these and others seriously contend that 'dynamics, or structure, or function, or change, or movements, or trends can be found outside of history—outside of observed events or acts in time and place situations.'

He goes on to say that 'the repeated experience of Western scholars over more than 2,500 years demonstrates unequivocally that the analogy between society and an organism has worked uniformly to stop inquiry and to produce images of social process plainly contradicted by evidence at hand.' His conclusion is that we cannot tackle problems by juggling bloodless concepts. 'Sooner or later the richly varied world of concrete experiences must be accepted and struggled with and conceptually subdued. . . . Our situation is such that we *must* hope that we can find some regularities in histories to guide us on our perilous way.'

RAGLAN

**Readings in Anthropology.** Edited by E. A. Hoebel, J. D. Jennings and E. R. Smith. New York and London (McGraw-Hill), 1955. Pp. xiv, 417. Price £1. 17s. 6d.

**179**

This collection of 51 extracts from books and articles is designed as a supplement to any general textbook of anthropology, and in particular to Hoebel's *Man in the Primitive World*. It is intended to provide a sample of the 'variety and vividness' of anthropological writings, and more especially to introduce new

lines of work. The editors hope that the book will be useful for large classes of beginners, where libraries are small.

The pieces are arranged by subjects, and although there is a preponderance of writings from and about America, the bias is not excessive. One of the most successful sections is that on physical anthropology, which contains six lucid, modern monographs on genetics, evolution and racial classification. There is a good range of articles describing social customs, and various writings on general theory, including a fine review by Lowie of the work of Morgan and Tylor.

But between the purely descriptive passages and the thoughts on general principles, the editors might have found room for more comparative studies of limited topics, like Wissler's account of the influence of the horse in the Plains, to illustrate precise scholarship as distinct from direct reporting or theoretical reflection. In the technology section, for example, one of the two descriptions of shifting cultivation could have been omitted to leave space for a monograph by Balfour.

Moreover, following the excellent accounts of the collaboration between genetics and physical anthropology, might there not also have been included just one example of the modern use of physics in studies of primitive technology? Ethnology is very thinly represented, and to show that the subject is still alive, some historical study from Africa or the Pacific would have been in place, perhaps a review of Heyerdahl's theories, which get a cavalier dismissal from Linton in his 'New Light on Ancient America.' There are quite full sections on field techniques and applied anthropology, and two articles on linguistics, but nothing on primitive art or folklore, and no hint of the classical tradition of Frazer, Hasluck, Dawkins and Gilbert Murray.

Outweighing these omissions, however, is the one great merit of the collection, that all the pieces are written in clear and fluent language. The editors have done well to show that the best anthropologists can still explain their work, however technical, in words intelligible to all.

W. C. BRICE

**Primitive Art.** By Franz Boas. New York (Dover Publications), 1955. Pp. 376. Price \$1.95.

**180**

This is an unabridged edition of the classic work on the development of art style first published in Oslo in 1927. All of the illustrations have been reproduced and a handy index of names has been added. The endpapers, which are new, detract more from the total appearance of the book than I would have deemed possible. The content is, however, the important thing, and since this remains unchanged we must be grateful to Dover Publications for making the book available again, and at a reasonable price. It goes without saying that no one can work in the fields of primitive art, art history, or the theory of cultural development without familiarizing himself with the contents of this anthropological landmark.

M. W. SMITH

## AFRICA

**The Cultures of Prehistoric Egypt.** By Dr. Elise J. Baumgartel. Revised Edition. (O.U.P.) 1955. Pp. 122, 13 plates. Price £2 10s.

**181**

The author's knowledge of her subject has evidently been gained from books and museum specimens only, and within those limits her work is excellent and shows long and careful research. On the other hand, her want of personal knowledge of the countries and peoples with which she deals has led her into many misconceptions and misunderstandings. To deal adequately with primitive cultures of the past, it is necessary to have an intimate knowledge of the primitive cultures of the present.

The book is divided into two parts. The first part contains a general survey of the whole period from the Badarian to the beginning of history. This is the weakest part of the whole work. The author casts scorn on those who have suggested that there was any culture in the Delta, and tries to prove that the early cultures of Egypt came from the South only. She implies that the Delta at that time was one vast uninhabited swamp, ignoring the fact that the seven branches of the Nile formed as convenient waterways for the

introduction of culture from the North as the single stream of the river from the South. Another point which she has not taken into account is that our knowledge of predynastic Egypt is derived entirely from excavation, and that these excavations have been made only within the last 70 years and always in uncultivable land, while the Delta during those 70 years has been thickly populated and fully cultivated, and therefore has not been available for excavation.

Dr. Baumgartel has unfortunately adopted the unscientific and misleading German nomenclature for two of the predynastic cultures. In European prehistory the French, with their usual clarity of exposition, have called each culture by the name of the place where the most characteristic objects were found, using numerals only where it could be proved that one form has developed from another, e.g. La Tène I, La Tène II. But to use Nakada I and Nakada II for two such distinct cultures as the Amratian and the Gerzean is a misuse of an established scientific method.

In the second part of the book Dr. Baumgartel is at her best, and her best is very good. Here she deals with the foreign connexions of

different cultures, and in this her knowledge of predynastic pottery, obtained not only from books and photographs but also from the handling of innumerable museum specimens, makes her book of value. In her study of pottery, she has confined herself to the decoration and the interpretation of the designs, and has little or nothing to say of the material (the clays) or the method of making. Also she does not distinguish between (a) slip decoration, i.e. the use of a solid material laid on the surface of the vessel, a method used chiefly in the Amratean (Nakada I) culture, and (b) the use of a liquid stain (manganese) which sinks into the substance of the pot, which is found in the Gerzean (Nakada II) culture. Both methods are called 'painting.'

As an intimate knowledge of pottery is the foundation of archaeology, Dr. Baumgartel could render a great service to Egyptology if she would produce a special study of predynastic pottery. If she would add to her present experience an expert knowledge of the materials and methods of making of ancient pottery in general, she might produce a book which could become the standard work on the subject.

Dr. Baumgartel has found, as do all students of predynastic Egypt, great difficulty in identifying many of the Nakada finds. Some years ago when I was making a special study of predynastic pottery, I asked Sir Flinders Petrie for the loan of the notebooks in which the two excavators had kept the daily record of the finds at Nakada and Ballas. He then told me that the notebooks had disappeared completely; though searched for, they were never found. The loss of those notebooks is one of the disasters of Egyptian archaeology.

M. A. MURRAY

**A Handbook of Tswana Law and Custom.** By I. Schapera. Second edition. O.U.P. (for Internat. Afr. Inst.), 1955. Pp. xxxi, 328, 1 map. Price £1 10s.

**182**

Professor Schapera's study of the social and legal institutions of the Tswana of the Bechuanaland Protectorate, published in 1938, represents the descriptive method for the study of primitive law at its best. The present volume is an unrevised reprint of the first edition. The original work rested on intensive field studies among the Kgalagadi and Ngwato tribes in 1929-35. Subsequent to 1938 Professor Schapera extended his field researches among the Tswana to include the Ngwaketse, Kwena, Tawana, Tlökwa, Malete and Rolong tribes. Local variations in law, discovered in these later researches, are not of great enough significance to warrant revision of the original text. Such variants as do occur are indicated by the author in the preface to the new edition. Recent trends in the modification of Tswana law are also touched upon in the second preface, so that the data of the volume are placed in the context of contemporary social conditions. In scope the volume embraces the elements of Tswana social structure, tribal constitution and substantive law. It includes highly informative data on the functioning of the age sets. Particularly interesting is the exposition of the integration of monarchy with family group, ward, village, district and section units of social structure. The Tswana had clearly solved their major problems of socio-political organization with genuine effectiveness.

The greater part of the volume is given over to a functional description of Tswana substantive law—both private and criminal. Professor Schapera's coverage of substantive content is simultaneously broad and detailed, the product of intensive familiarity with the subject by a highly competent and skilled observer.

As a method of investigation in primitive law the descriptive approach is definitely superior to what may be called the ideological. In the latter, which was the main method of research used by earlier scholars, reliance is given to summary statements of legal norms as formally conceived by the people under investigation. Law, however, is always much wider in its scope and variety than the idealized norms indicate. Indeed, law-in-fact is often quite different from the idealized version enunciated in first responses to such questions as, 'What is the law covering theft?' The descriptive approach, on the other hand, as used by Schapera is based on intimate observation and multi-dimensional inquiry. It is much more sensitive to the real facts of legal life. From such an account as set forth in the *Handbook*, the reader is able to derive a highly accurate understanding of what

is legally expected of the Tswana in their inter-personal and inter-familial intercourse.

As is characteristic of legal studies relying on the descriptive method, Schapera's *Handbook* offers only a slight amount of data on procedural matters. Thus, we learn a good deal about what the law is in its more formal sense, but we are not shown the law in action or application. No more than two slight cases are included in the text. Nor does this volume undertake a jurisprudential analysis of the Tswana legal system, or engage in comparative examination of Tswana institutions or practice. The nature of Schapera's task precluded the possibility of inclusion of interpretation for scientific purposes. It is not that the author is unaware of the wider implications of his data. Rather, he undertook his researches and wrote this book under official auspices for the guidance of administrative officers. He did not feel authorized to do more than meet their needs in the publication under review, and that obligation has been filled most admirably. Therefore, the reissue of the volume in this new edition is most welcome, for the original edition has long been out of print and unobtainable.

In several separate monographs or articles Professor Schapera has explored other aspects of the legislative and judicial process among the Tswana to good effect. Perhaps he will in the not too distant future bring forth a re-examination of Tswana law in terms of the collection of case materials which he himself has compiled, or which have become available in the court records of the tribes since 1935. This, and a social-science analysis in terms of comparative legal dynamics, would constitute a most valuable addition to the excellent contribution represented in the present volume.

E. ADAMSON HOEBEL

**A Maasai Grammar.** By A. N. Tucker and J. Tompo ole Mpaayei. London (Longmans), 1955. Pp. viii, 370, vocabulary. Price £1 10s.

**183**

The pastoral Maasai of Kenya and Tanganyika, whose predatory habits and success in implementing them ensured early contact with Europeans, have continued to occupy an unusual place in their affections. The language, however, perhaps owing to its complexity, has received little serious attention, and it has been left to the authors of the present work to follow up the early pioneering work of A. C. Hollis (*The Masai: Their Language and Folk-lore*, 1905). From such an observation the Missions must, of course, be excluded: their work has been continuous but primarily concerned with the practical need for communication, and it is with this need that the recent *Twelve Lessons in Masai*, by R. T. Shaffer, is primarily concerned.

The work under review must, therefore, be welcomed heartily, as the most detailed study of the language to date. Not only is attention given to the significance of tone, but also to vowel quality; and the orthography adopted is that already recognized for related languages of the Southern Sudan and Uganda.

I can claim no competence in the language, and no evaluation of the Maasai used will here be offered.

The work is divided into three parts. Part I introduces the student to Nouns and Adjectives, in the orthography approved for use in schools and for textbooks. In Part II, wherein the verb system is treated, variation in vowel quality is noted by variation in the type face. Thus, 'close' vowels continue to be printed in normal type while 'open' vowels are printed in heavy type. In this part, too, the authors note that 'tone diacritics are added where serious ambiguity would otherwise exist . . .' From the extent to which this appears necessary, it is clear that tone presents a serious problem for the learner. Part III is concerned exclusively with tone, and introduces nothing new grammatically. This, it is suggested by the authors, may be omitted by anyone not interested in this aspect. However, it seems clear from Part II that the chapters on Tone must be of more than academic interest even for practical students. Finally, as Appendices, there is a Maasai text, and a useful Maasai-English/English-Maasai Word List, the items of which are taken, for convenience, exclusively from the grammar and the Companion Reader (*Inkuti Pukunot oo !Maasai*, by J. Tompo ole Mpaayei *Annotated African Texts*, III, O.U.P., 1954). Throughout the work the authors have avoided any innovations in grammatical

terminology, a fact which will be greatly appreciated by those using the book as a textbook.

So far as can be seen, it is nowhere stated for whom the work is primarily intended, though from the arrangement of the material and from references in the text, it is clearly intended to serve the needs both of the practical student and the specialist. This, it is suggested, is over-optimistic. The majority of those anxious to learn the language—if indeed there be more than a few enthusiasts—are probably concerned with practical issues, and for such people the treatment, particularly in Part II, seems unduly compressed. Shorter chapters and more exercises would be a benefit, especially in the long chapters on derivatives and compound derivatives with only five exercises between them. For all except the most highly educated African students some simplification seems desirable, and since, to my knowledge, there is no provision for the vernacular in the higher levels of education in Kenya or Tanganyika, the work seems likely to appeal only to a very limited number of enthusiasts, if the price proves no deterrent. The specialist, by contrast, will welcome particularly the tonal notes to the exclusion of other features, so that one wonders whether two books might not have served better the differing needs of students. One, a scientific description of the language for specialists; the other, a severely practical handbook for those for whom communication is a daily necessity.

Such considerations may, however, seem trivial when compared with the very real achievement of a work that must remain a standard reference book on the language for many years to come.

W. H. WHITELEY

**Ethnographic Survey of Africa: The Shona and Ndebele of Southern Rhodesia: The Shona.** By Hilda Kuper. **The Ndebele.** By A. J. B. Hughes and J. van Velsen. London, (Internat. Afr. Inst.), 1955. Pp. 131. Price 15s.

184 The Shona occupy about three-quarters of Southern Rhodesia and a large area of Portuguese East Africa. In 1931 their numbers were estimated at 825,000; there seems to be no later estimate. Mrs. Kuper has in 28 pages summarized the information contained in about 150 books and articles; she says at the beginning that 'inevitably in a brief survey such as the following, details and variations of custom are omitted, and a more uniform picture is presented than actually occurs.' It is difficult to see what purpose is served by so summary an account of so large and varied a tribe.

The Ndebele were estimated at 117,000 in 1931, and may now be between 300,000 and 400,000. One of the authors, Mr. Hughes, has checked in the field accounts given in the literature. The Ndebele are divided into three classes or castes; the first descended from Mzilikazi's early Nguni followers; the second from Sotho and Tswana who joined him on his way north, and the third chiefly of Shona conquered or captured in raids. The classes were formerly endogamous, and the authors tell us that 'inter-caste marriages are still frowned upon and are of comparatively rare occurrence' (p. 75). On the other hand, Mrs. Kuper says that 'among the Ndebele ... intermarriage between all groups is now accepted' (p. 29).

Although Lobengula only died in 1893, much of what happened in his time seems to have been forgotten; Mr. Hughes could not find out whether the regiments were organized on an age-grade basis, as among the Zulus, or whether there were seniors and juniors in the same regiment (p. 67).

We are told that nowadays the Ndebele live in rectangular houses, often of brick, use ploughs and wear European clothes, but that they 'still have a specifically "Ndebele" way of life, with a complex system of law, customs and morals, and they are liable to feel strongly about actions which offend against it' (p. 53). RAGLAN

**Ethnographic Survey of Africa: North-Eastern Africa, Part I: Peoples of the Horn of Africa—Somali, Afar and Saho.** By I. M. Lewis. London (Internat. Afr. Inst.), 1955. Pp. 200, 2 maps, text figures. Price 21s.

185 The greater part of this volume is devoted to a description of the Somali with a shorter section on the Afar tribes of the Danakil Desert and a summary of what little is known of the Saho. A valuable distribution map is included and an equally valuable bibliography. Full accounts are given of the distribution of tribal

groupings, physical environment and the main features of the economy of each group, as well as the main cultural and physical characteristics and religions.

The various Somali and Sab confederacies are described and their genealogies given. The members of these groups trace their descent from the common ancestor and culture hero of their group and this rather rigid genealogical interpretation of the origins of groups is the basis of Somali society. It is considered, however, that it might receive a more critical review from future investigators, and a study of blood groupings would probably be of assistance here. The book also brings out the need for proper archaeological investigation of the numerous proto-historic sites, especially those along the coasts, as these will most certainly provide important evidence relating to the introduction of new racial elements and cultural connexions with outside peoples, and will enable the origins and interaction of the existing groupings to be more accurately determined. The book constitutes an important and scholarly introduction to a study of the present-day populations of the Horn of Africa, and one may hope that it will provide the inspiration for initiating systematic sociological investigations among these most interesting peoples whose influence, as archaeological investigations begin to show, has in the past been felt far to the west and south of their present habitat.

J. DESMOND CLARK

**Ethnographic Survey of Africa: Peoples of the Niger-Benue Confluence.** By Darryl Forde, Paula Brown and Robert G.

186 Armstrong. London (International African Institute), 1955. Pp. 158. Price 16s.

This volume of the Ethnographic Survey deals with the Nupe, Igbira, Igala and Idoma-speaking peoples. They live in an area which is described as having been for a long time 'a focal area of migrations, commerce and conquest, which have set up cultural cross currents of many kinds.' The first section of the volume, written by Professor Forde, is concerned with the Nupe, certainly one of the most fully described of all West African peoples. The thoroughness of Nadel's accounts, upon which this section mainly relies, here makes possible a systematic and concise review of Nupe culture and social organization which is especially valuable within the general context of the Ethnographic Survey. The second section, written by Dr. Paula Brown, deals with the Igbira and is based upon the far more limited accounts mainly written, it would seem, by administrative officers, and includes much unpublished material. The final sections, on the Igala and the Idoma-speaking peoples, have been written by Dr. Armstrong whose own fieldwork was done amongst the Idoma and who has some personal knowledge of the Igala. His account of the latter people is based mainly upon written sources and he has also been able to draw upon these, including a large number of unpublished reports, for his section on the Idoma-speaking peoples.

The interest of this volume may be expected to lie particularly in its two final sections. These unfortunately are disappointing. The published material on the Igala is admittedly slight, but the accounts of their kingship are fuller than the present summary would indicate. Clifford's description of this ('A Nigerian Chiefdom,' J. R. Anthropol. Inst., Vol. LXVI) suggests many points of comparison with the more fully studied kingship of the Nupe: the importance of a ruling or superior class and the central position of the Ata in the 'state' organization; the system of titled offices and of groups associated with the Ata; the balances and checks involved in the kingship, both in the mode of succession and in the counterpoised office of Ashadu; the importance of process in the evolution of the kingship and in its relation to the society. Even on the descriptive level these features are not fully stated. Moreover, the summary offered occasionally misrepresents its authorities. On the question of the relation between the Jukun and Igala kingships, for example, it is stated (p. 80) that '[Clifford] admits, however, that there are today few points of resemblance between the two kingships,' some of the differences listed by Clifford then being named. This is in fact a reversal of what Clifford actually writes. Thus after noting these differences he concludes: 'To summarize, there is so much in common, despite the foregoing divergencies, between the Igala and Jukun kingships as to leave little or no doubt that the former is an

offshoot of the latter . . .' (*op. cit.*, p. 417). This statement of resemblance is substantiated within Clifford's article by his account of the rites and ceremonies associated with the Igala kingship. Other weaknesses in Dr. Armstrong's summary could be named. It may be noted that four references given in H. A. Wieschhoff's *Anthropological Bibliography of Negro Africa* are not included in the Igala bibliography, here merged with that of the Idoma. One of these (F. F. W. Byng-Hall, 'Notes on the Okpoto and Igala tribes,' *J. African Soc.*, 1908, pp. 165-74), besides giving some subsidiary details concerning the kingship, provides some highly relevant facts concerning the judicial organization and a few points concerning kingship, a subject which has been elided in the present account.

It would be unfair to criticize the final section on the Idoma-

**The Sociology of an English Village: Gosforth.** By W. M. Williams. London (Routledge & Kegan Paul), 1955. Pp. x, 246, 12 maps, text figs. Price £1.5s.

187 Although anthropological pundits are continually stressing the need for study of our own society, very little has hitherto been done; sociologists—in whose domain such studies are often said to lie—in Great Britain have either concentrated on problems of a specialized nature or on theoretical definition, and such generalized descriptions as exist relate to urban situations. 'Rural sociology' has never been recognized here as a separate field of enquiry, and hence the only anthropological studies of rural communities in the British Isles have been written by foreigners or by geographers. (I am thinking in particular of two classic studies, both of peripheral areas: Arensberg and Kimball's pre-war study of West Clare, *Family and Community in Ireland* (New York, 1939), and Rees's study of a Montgomeryshire village, *Life in a Welsh Countryside* (Cardiff, 1950).) Mr. Williams is also a geographer by training, but he has clearly made an extensive reading of anthropological literature. And although he is not writing about his home region, nevertheless he has acquired a deep insight into the local history and background in a way that a foreigner would find difficult. Thus he is able to cite much historical information from medieval documents which would normally be overlooked by anthropologists—if indeed they were aware of their existence—although this material provides interesting diachronic substantiation of the author's theories.

The present study relates to the West Cumberland parish of Gosforth—an area of some 7,124 acres containing a population of 723, over two-thirds of whom were born within a 10-mile radius of the parish. Ecologically the region occupies an intermediate position containing elements of both an upland sheep-farming economy and lowland dairying, but the first tractor did not reach the area until 1939. Whilst therefore there are several features which suggest that Gosforth is atypical of the English rural community, its comparative isolation and the stability of settlement have facilitated this type of examination.

The author gives us a detailed description of the economy, life cycle, social-status groupings, formal and informal associations and religious observance of the people of Gosforth, as well as an interesting chapter on the significance of neighbourhood in the area; but he is primarily concerned with demonstrating the dynamics of kinship and social status, which, he shows, predetermine the course of all social activity. Nevertheless the study is wrongly titled: Williams is too anxious to provide the reader with data from cultural anthropology for the work to fall conveniently into the category of sociology: it is a socio-ethnographical description (if one must eschew the word 'culture' in a title), and even with this reservation he has occasionally succumbed to the perennial temptation of all authors, the recording of irrelevant material (*cf.* the note on popular conceptions of the influence of the moon, p. 234, note 18). The most stimulating feature for me, however, was in his disregard for orthodox anthropological shibboleths such as the neglect of folklore.

Yet there is much of interest even to 'structural' anthropologists: for example he shows that the mechanization of agriculture has decreased the dependence of the younger generation since it is they who are largely responsible for the new implements, whilst his new

speaking peoples since most of the material offered here is new and would appear to require the longer and more elaborate treatment that is possible in a monograph but cannot be given in an Ethnographic Survey. As it stands, the final impression of this section is confusing, its chief weakness being perhaps the inadequate definition which is given to many of the broader terms or concepts used ('Idoma,' 'Akpoti,' 'Land,' 'Chief,' etc.).

One is nevertheless grateful to this volume for bringing together some of the material upon four peoples who, despite—and even because of—their diversities of social organization and the diverse historical influences which they have been subject to, offer an interesting field for comparative study, particularly regarding the notions of kingship and the structural importance of this institution within emergent or developed 'state' societies. M. J. RUEL

## EUROPE

method for the determination of social class (outlined in a separate appendix) appears to be much more workable in a rural situation than that of Lloyd Warner. Throughout the book Williams accentuates the extent to which the community is changing through the arrival of outsiders with differing value systems—values which are usually associated with urban culture—although he unfortunately does not undertake an examination of the content of these new values.

Perhaps the most important lesson to be drawn from *The Sociology of an English Village: Gosforth*, however, is the fact that there is much interesting material on our very doorstep, waiting for the arrival of the anthropologist, but rapidly losing its local individuality. The study of such communities can be conducted at a fraction of the cost of many fieldwork trips to more exotic climes. The interpretation of material may well be subject to greater difficulties, but as Mr. Williams has shown, these can be overcome, and with the growth in volume of such studies, rural sociology—whether or not as an independent discipline—will be established.

IAN WHITAKER

**Internationale Volkskundliche Bibliographie: 1950-1951.** Edited by Robert Wildhaber. Basle (Librairie-Éditeur S.A.), 1955. Pp. xxxi, 664.

188 The latest issue of the trilingual International Folklore Bibliography, published by the Commission Internationale des Arts et Traditions Populaires under the auspices of U.N.E.S.C.O., faithfully follows the patterns of its predecessors. This volume, however, contains rather more material from Eastern Europe, giving some fascinating glimpses of work in progress there; thus several entries (*e.g.* 1222, 1246, 1251) show that several of these countries are alive to the advantages of questionnaires.

The entries relate to published work on all aspects of *Volkskunde*—the only two European countries missing are Roumania and Albania—but, as the Editor points out, the problem of whether to include references to the folk-culture of European colonists in other continents is not easily solved; in this volume South Africa, Canada, and the U.S.A. are liberally represented but other groups are entirely missing. Whilst most British anthropologists will regret the distinction between *Volkskunde* and *Völkerkunde*, it is readily admitted that some regional limitation is essential. The 8,380 references in this issue are conveniently and simply arranged, but they represent a maximum if the volume is to remain usable by the private scholar.

The only regret which I have is that the British contribution to these studies must perchance be represented by so much uneven and often dubious scholarship. The prolificity of entries from other and smaller countries reflects a less one-sided understanding of 'the study of man.'

IAN WHITAKER

**The Coloured Quarter: Negro Immigrants in an English City.** By Michael Banton. London (Jonathan Cape), 1955. Pp. 254, maps, index. Price 21s.

189 Since Dr. Little published his *Negroes in Britain* (1947) as a pioneer anthropological work, there has been a number of other anthropological and sociological studies of this kind. Dr. Banton's Negro grouping was that which lived in Stepney; and he has produced a thorough and well written study of it. Here is no

attempt to consider a general problem, such as Dr. S. Eisenstadt has made in his *The Absorption of Immigrants* (1954)—a comparative study based mainly on the Jewish community in Palestine and the State of Israel. Dr. Banton has set out to give an account of the history of this coloured quarter and its relations with its neighbours (this history is very concise), of how and why the immigrants come to Britain, and of how they settle down, enter into British life, and form their own groupings and relationships. He ends with 'Conclusions for a Social Policy.'

The data on which the study is based were collected partly by Dr. Banton himself and were drawn partly from published and unpublished sources. They are clearly presented, and the whole

makes a good and interesting analysis. Any anthropologist interested in the development of social relationships, and the manner in which what was haphazard becomes systematized, will derive much from it. I wish that after he had finished this study, Dr. Banton had moved to investigate the receiving British community, instead of moving his field to West Africa. It is to this point, surely, that students of Negro immigrants must now shift their attention. Each Negro community studied adds to our knowledge—and Dr. Banton's does so notably. But the main problem, both for anthropology and for social workers, lies on the other side of the colour line. It is obvious that Dr. Banton could have done important pioneer work there.

MAX GLUCKMAN

## CORRESPONDENCE

### Trobriand Island Clans and Chiefs

**190** SIR,—Increasing literacy in English among Melanesians now threatens alien fieldworkers in that area with a new occupational hazard.

One day in 1955 at Port Moresby a slight young man stopped me in the street and spoke to me, 'Excuse me, sir. Are you an anthropologist?' he asked. I said that I was. 'Yes, so I understood,' he said, now smiling at me as if we had just shared a delightful secret. 'You have presumably read Malinowski,' he added. Again I said 'Yes.' 'Then, sir, you may be interested to know that Malinowski was in error,' the young man said. 'I am a Trobriand Islander myself, and from what I have heard of his writings it is clear that Malinowski did not understand our system of clans and chiefs.'

My new acquaintance then pulled a typewritten page of foolscap from his pocket. 'Here is a short account I have written myself,' he said. 'It outlines the facts as they really are, and I should like you to have it.'

'What do you want me to do with it?' I asked, for something in his manner suggested that he was seeking my aid.

'I should be much obliged if you would make the facts available to those who have been misinformed,' the young man answered. He spoke as if by doing what he asked I should also be doing him and his people a great kindness.

I promised to do my best for him, and to fulfil that promise I append the statement which he gave me. It was written in English and it is exactly reproduced hereunder. It should be compared with *Argonauts of the Western Pacific*, pp. 62-72. MURRAY GROVES Balliol College, Oxford

### INDEX OF THE PERMANENT POSITION OF CHIEFS (GWEGUIA IN TROBRIAND ISLANDS)

There are four kinds of clans (KUMILA) in the Trobriand Islands. Here are the names of the clans (KUMILA):—

1. MALASI— In this clan the highest position is first class (TABALU), only one position.
2. LUKOSISIGA— In this clan the highest position is second class (TOLIWAGA) and the lowest position is (KWAINAMA) third class only two positions.
3. LUKUBA— In this clan the highest position is fourth class (TUDAVA) mid position is fifth class (MWAULI) and the lowest position is sixth class (MULABWAIMA) only three positions.
4. LUKULOBUTA— In this clan the highest position is seventh class (KAITOTU) only one position.

In each clan (KUMILA) have position of chiefs (KESISULA GWEGUIA). Here are the names of position:—

1. TABALU	first class of Chief
2. TOLIWAGA	second .. ..
3. KWAINAMA	third .. ..
4. TUDAVA	fourth .. ..
5. MWAULI	fifth .. ..
6. MULABWAIMA	sixth .. ..
7. KAITOTU	seventh .. ..

From first class to seventh their general names are (GUIAU—SINGULAR, GUMUGUIAU—PLURAL).

8. KARAIWAGA and KADADELE VALU..... Parliament
9. KIWATASI GWEGUIA and TOSIKWAWA.... Official Secretary

### 10. TOKAI OR VILOMUGWA.....

That is a common position From first class to seventh they had their sign by decoration, but it is not the same decoration as other, it would be different in each class. Every position knows what to decorate. But they must decorate themselves in order, but if it is not they will get trouble from other classes. In each position have word (LUTOMOIA) that means first born, the word of (LAGWADI) that means second born. The first born always to re-inspect, the second born when the day is will be decorate the house dancing and other particulars of reason. The second born (LAGWADI) must be always asked for permission of decoration from first born (LUTOMOIA), the second born cannot be decorate anything without an authority from first born. The first class which we called (TABALU) is first born he can have more decoration and he can have more than twelve wives. The second class which we called (TOLIWAGA) that he can have six or four wives and the decoration is not more than the first class. The other class they can have two wives only and decorations but still be the chief position, (GWEGUIA). There are only No. 10 position which we called (TOKAI) or, (VILOMUGWA) they have nothing on decoration and they cannot have more than one wife. Every chief in Trobriand Islands are need to have more wives not just for their love. But the greatest reason is this, they need more wives that can be more relatives to make gardens and to supply plenty of yams, pigs and betelnuts to their chief every year. So that the chief can be happy to make big feasts and dancing for his people. Because the chief he knows he had quite enough yams, pigs and betelnuts to spend on the ceremonies or another particular reason come through to his village. But if he has not enough yams and other articles he would be very ashamed and also his relatives they will be very ashamed too. Because they are not working hard to save their chief. The chief should give a payment to his relatives if they are working good for him. The payment was black flat stones armshells and money string shells. Every year the chief his relatives they will filled the yams in their chief his food house we called (DADODIGA)—on that day the chief will kill plenty of pigs and to prepare good food for his relatives. Because they are working hard for their chief.

By Lepani Watson

### Nomenclature in Indian Archaeology: 'Togau' or 'Niaain.'

**191** SIR,—Recently, in *Relative Chronology in Old World Archaeology* (p. 62), D. E. McCown has attempted to supersede the name 'Togau ware' with that of 'Niaain ware.' Why it is to be preferred is hard to see. This ware has been known for a very long time; Stein found it at a number of sites in southern Baluchistan, and Majumdar similarly in Sind, but the recognition of its importance and its establishment as a specific industry by Miss de Cardi was 'original,' not 'later' as stated by McCown. The report of Stein's reconnaissances in Las Bela, already long delayed, has yet to be published, and there is nothing to show that he ever coined the term 'Niaain ware,' which was apparently thought up by McCown quite recently. The acceptance of 'Niaain' seems to be quite unjustified. Sometimes a fresh nomenclature can produce order and clarity, but this clarifies nothing and merely imposes an uneuphonious whinnying substitute for the easily spoken, easily spelled term 'Togau' which has in any case been accepted by those who work in this field.

Hingham, Norfolk

D. H. GORDON



(a-c) Cachalot-ivory pendant figure of Haapai type



(d, e) Cachalot-ivory hooks with Haapai-type images



(f) Hardwood image of Fijian type

## HUMAN FIGURES FROM TONGA AND FIJI

Photographs: T. T. Barrow



(a) Image, Fiji



(b) Oil dish, Fiji



(c) Image, Western Samoa



(d, e) Food-hangers, Fiji



(f) Head of image, Fiji

## HUMAN FIGURES FROM FIJI AND WESTERN SAMOA

# HUMAN FIGURES IN WOOD AND IVORY FROM WESTERN POLYNESIA\*

by

T. T. BARROW

*Dominion Museum, Wellington, New Zealand*

**192** The first purpose of this contribution is to make available to students important figure material from Western Polynesia in the Fijian collection of the University Museum of Archaeology and Ethnology, Cambridge. The record is intended to supplement research on Tongan images by the late Sir Peter Buck.<sup>1</sup> The second and more original purpose is to formulate the essential characteristics of figure representations in Fiji, and to differentiate them from figures of Tongan manufacture. Some confusion of identity has arisen because Tongan images have been collected in Fiji.<sup>2</sup> Further confusion has resulted from the misconception that human images do not occur in Fiji, or as one writer has said: 'The Fijians did not, at least within recorded times, ever use images in human form.'<sup>3</sup> The lack of published type specimens has not helped museum authorities or private collectors in identifying Fijian figures when they have been acquired without data.<sup>4</sup> The Cambridge images from Fiji, with the excellent Tongan type series figured by Buck, *plus* the British Museum image from Western Samoa illustrated here as Plate Mc, provide us with specimen types from the three points of the Western Polynesian triangle.

After description and discussion of specimens figured, a comparative analysis of Fijian and Tongan images will be given.

*Plate La-c.* No. 25.336: female figure pendant, carved from cachalot-tooth ivory. Overall length 10·8 cm.; width (at shoulders) 4·5 cm.; depth (at breast) 2·7 cm. The figure was collected in Fiji before 1900. However, on the basis of Buck's evidence figures of this type (including those incorporated in the suspension hooks, *Plate Ld, e*) may safely be attributed to the Tongan Islands, and probably to a school of craftsmen working in the Haapai group. The stylization of the figure is quite different from that of the necklace figures shown in fig. 1. It is suggested here that this necklace represents a Fijian version of the Haapai-type figures in ivory.

The near-naturalism of this pendant figure illustrates the unique style of the Haapai carvers, paralleled elsewhere in Polynesia only by some Hawaiian female figures.<sup>5</sup> Usually the sex of Oceanic images is marked by genitalia, but in the Haapai images there is full development of secondary sex characters of woman, *i.e.* full breasts, curved abdomen, and enlarged buttocks. The exaggerated buttocks suggest steatopygia, but in fact this condition was not present in Polynesia, although heroic proportions were cultivated as an ideal of beauty in woman. The Tongan craftsman has ventured as near to nature as he could or cared to, for the

broad square shoulders, narrow hips and heavy thighs are merely exaggerations of the prevailing physical type of woman in Western Polynesia.

A small lug set to one side of the head suggests that this figure was worn singly as a pendant. It conforms to a Polynesian type of ancestor-pendant series seen in Easter Island, the Marquesas and New Zealand. At first glance it would not appear to bear any relationship with the well-known Maori *tiki* form, yet in many respects the two are parallel.

Reasonably conclusive evidence in support of Tongan provenance for this type of female figure in ivory is provided by a specimen in the Cook collection of the Museum für Völkerkunde, Vienna. This example, No. 146, is of whale ivory, the length being 6·2 cm. First illustrated by Ratzel, this valuable item has been recently described by Dr. Irmgard Moschner as fig. 105 (p. 229) in the following work: 'Die Wiener Cook-Sammlung, Südsee-Teil,' p. 136, *Archiv für Völkerkunde*, Vol. X (1955). Another ivory illustrated by Dr. Moschner as fig. 106 (p. 229) in the same work, closely approximates to the lower part of the suspension-hook god symbol illustrated in *Plate Ld*.

*Plate Ld.* No. 55.247: suspension-hook god symbol carved from a cachalot tooth. Height 12·3 cm., the two figures approximately 6·1 cm.; width (at heads) 5·3 cm., (at base) 4·6 cm. The specimen was presented to Sir Arthur Gordon (later Lord Stanmore), then Governor of Fiji, about 1875, by a Fijian called Tavita, who assured him that it was the 'Nadi Devil,' or idol of the Nadi district. Lord Stanmore, son of the collector, deposited the hook in the University Museum of Archaeology and Ethnology in 1955.

As in *Plate La-c* and *e*, the figures indicate that the specimen is of Tongan origin, from the region of the Haapai group. Of nine light suspension lugs, two have broken away; otherwise all parts are in splendid condition. The colour is the rich amber so much treasured by Fijians, but usually bleached out by continual exposure to light. A string of red, white and blue beads of a type common in Tongan decorative work of the early contact period is attached as a suspension cord.

Of three known hooks of this type,<sup>6</sup> this is undoubtedly the most important because of its splendid documentation. Theodor Kleinschmidt<sup>7</sup> provides us with information received from Sir Arthur Gordon. This note is so interesting and important that it is here given in full:

GOVERNOR SIR ARTHUR GORDON'S NOTE TO THE IDOL FROM THE  
NADI DISTRICT (VITI-LEVU)

Tavita tonight produced and gave to me the 'Nadi Devil,' the idol of the Nadi District. It consists of an ivory—cut out of whale's tooth and representing two women back to back with nine hooks below also of ivory.

\*With Plates L and M and a text figure. Publication of this article has been assisted by a grant from the Trustees of the Emslie Horniman Anthropological Scholarship Fund, under whose auspices Mr. Barrow is conducting research at Cambridge.

This figure represents 'Na Lila valu' the double wife of . . . the Chief God of Nadi, by whom it was given to an old priest . . . of . . . as a Vakadimadina of his priesthood. It or they was or were deposited in a small 'bure' enclosed in a large one of ordinary description. Into the smaller one no man was allowed to enter. It had two doors and the figure stood generally facing both during the day, and was laid in a basket to sleep at night. It stood of itself during the heathen days but since the 'lotu' it fell down and could not be made to do anything it had done before. In olden time it spoke with a thin squeaky voice. Luki has himself heard it when he was a boy. He was passing the 'bure' with a number of some other boys and saw the little figures standing at the door and calling after them in a thin little squeaky voice 'Maiivi ko lako?' and adds he saw them wave their hands. Though generally standing in the 'bure' they would walk about it and sometimes were to be seen at the door and sometimes up in the roof. They detected thefts, naming the thief if properly supplicated to do so, and when they wanted food, they would name a man and squeak out: So and So has not given me food lately, if he does not give food he will die of the 'Lilu.' When the Lotu had come and the virtue of the charm had departed, the image was hidden by the priest under a post in the house. There Nemani found it hidden in a hollow coconut-shell. He kept it at Nawaka and Tavita seems to have appropriated it from him for me.

The small 'bure' mentioned is possibly the type of small sennit temple found in many museum collections.<sup>8</sup> The ability to detect thefts and the naming of the thief, with demands for food offerings, suggest priestly craft at work, e.g. the 'thin squeaky voice' sounds suspiciously like ventriloquism. But with the coming of the 'Lotu,' that is, the church, the *mana* of the charm departed. The whole account is excellent, and from it we may class this suspension hook with the god symbols.

*Plate Le.* No. Z 2740: suspension hook carved from a single large cachalot tooth. Length 17·8 cm.; width 7·7 cm.; depth 4·8 cm. The specimen was collected in Fiji. Kleinschmidt<sup>7</sup> describes and illustrates the specimen as an 'idol of whale's tooth, in possession of the States Secretary, J. P. Thurston.' The hook differs from *Plate Ld* in size, arrangement of suspension lugs and colour. In type it is nearer to a third known hook of this form, in the collection of Marischal College, Aberdeen. All three hooks have the same back-to-back Haapai-style females, and we may assume that the documentation of *Plate Ld* is evidence that this (and the Marischal College hook) are also god symbols.

*Plate Lf.* No. Z 2812: female figure, carved in hard-wood; black patination. Overall height 33 cm.; width 9 cm.; depth 5·5 cm. Collected in Fiji by Baron von Hügel about the year 1875. The index card for this specimen gives the Fijian name as *makatau*, meaning 'idol,' but the note adds that this was probably a missionary term.

This figure may be accepted as typically Fijian. The stylization combines Melanesian with predominating Polynesian elements, yet the figure lacks the near-naturalism of Tongan images. A notch under one foot suggests that the figure was formerly attached to a base, which would have enabled it to stand freely.

*Plate Ma.* No. Z 2869: female figure cut in light brown wood. Overall height 52 cm.; width (at shoulders) 16 cm.; depth (at head) 11·5 cm. The figure was given to Baron von Hügel about 1875 at the Rewa River, Viti Levu, we

are told, by 'a pretty half-caste, "kai-loma" girl at (Bau) Mbau, as her portrait.' The heavy split up the centre of the image suggests that unseasoned wood was used, and therefore it is likely that the figure is later than others in this series. The stylization is strongly in the New Caledonian manner.

*Plate Mb.* No. Z 3431: oil dish in human form. Kingsley Bequest. Length 46·5 cm.; width 28·5 cm. The dish is in perfect condition, rich walnut in colour, with a ripple of lighter grain. A suspension lug occurs above the head. Two supporting legs on the base are in a position which suggests that they represent the nipples of the breast. The transposing or inverting of anatomical features, for example in turtle-form bowls, is common enough in the Fiji-Tonga area, and might well be present in this instance. Several other examples of human-form dishes from Fiji are to be found in museum collections, but it is doubtful if any is as large, fine, or well preserved as this specimen.

*Plate Mc.* Image from Western Samoa. British Museum collection. The specimen is included here as a comparative example from the third sub-area of Western Polynesia. The measurements are: length 69 cm.; width 22 cm.; depth 15·5 cm. This is an especially rare figure, for it is the only known example from the Samoan group. It was presented to Queen Victoria by King Malietoa of Western Samoa in the year 1841. First figured by Kraemer,<sup>9</sup> it was later considered by Buck, who was of the opinion that the image 'was made locally in Samoa by an inferior artist who was either Tongan or someone who was acquainted with the general appearance of the Haapai images.'<sup>10</sup> This appears the best explanation, for, as Buck says, it is more feasible to accept diffusion from Haapai rather than independent creation in Samoa. The image approximates to the Haapai stylization, yet there appear to be other influences at work.

*Plate Md.* No. Z 3773: suspension hook with disc, incorporating male figure. This specimen was collected by Captain Knollys, who was apparently attached to the staff of the Governor of Fiji in the late eighteen-seventies. The hook was purchased from the Knollys collection in 1898 by Baron von Hügel, and later passed into the Cambridge collection. Overall length is 32·5 cm.; at base, 8 cm. A detachable wooden disc rat-baffle measures some 35 cm. across. The stylization of the figure is unique in the series. Considerable distortion appears in the placing of features: for example, breast nipples appear behind the arms, which themselves spring from near the ears. The stylization, while possessing Polynesian elements, is strongly Melanesian, an effect which is again heightened by the matt black patination.

*Plate Me.* No. Z 3775: suspension hook incorporating a female figure. Collected by Baron von Hügel in the locality of Na Vuavua, Rakiraki district, Viti Levu. The overall height is 42 cm.; figure 28 cm.; width at base, 11 cm. Records give the following particulars: 'Hook for hanging food basket, from Navasa Naitasiri—figure of a man from Vani bau, Navua, Viti Levu. Very hard to get them to part with discs which are tiresome to make.' The figure, however, is undoubtedly female. The stylization of

the head is of the type commonly seen on the well-known oil dishes of which Plate Mb is an example. The whole is heavily patinated a dense black, which heightens the impression of strong Melanesian elements in the carving.

*Plate Mc.* No. Z 2879: head cut from a large image. Collected by Baron von Hügel about 1875, in the locality of Matai Lombau, Wi ni Mala River, Viti Levu. The height is 20 cm.; width 16 cm.; depth 19·5 cm. The colour is dark brown to black. The museum records give the following note: 'This was part of a figure presented to the collector, name of donor unknown, and was separated from the body part on account of transportation difficulties.'

The stylization of the head (like that of the body parts of *Plate Ma*) is strongly New Caledonian in manner. The combination of traceable characteristics would appear to be a feature of Fijian figures in wood.

*Fig. 1.* No. Z 2752: necklace comprising eight cachalot-ivory female figures. Arranged alternately with the figures are nine 'tooth' units, also in ivory. All the elements are



FIG. 1. NECKLACE WITH HUMAN IMAGES, FIJI

suspended from seven sennit cords. The necklace is believed to have been presented to Lady Gordon in Fiji about the year 1884. Little information is recorded for this most interesting specimen.

Measurements in centimetres for the individual human images in the necklace from left to right (in the fig. 1) are as follows:

	(a)	(b)	(c)	(d)	(e)	(f)	(g)	(h)
Height	7·4	8·1	8·1	8·8	9·6	8·8	8·1	7·8
Width	3·2	2·9	3·2	3·3	3·4	2·9	2·8	2·8
Depth	1·6	1·7	1·7	1·7	2·8	2·3	1·7	1·6

On comparing the human images of this necklace with those figured by Buck, and with the other Cambridge examples, *Plate La-e*, it is apparent that the stylization is different. The figures are not of the Haapai type, but approximate rather to Fijian figures in wood. On the grounds of this similarity it is reasonable to assume that the necklace is of Fijian manufacture, and not Tongan in origin like most ivories collected in Fiji. If this reasoning is correct, the necklace must assume a place of special importance.

The comparative table given below is a tentative attempt, based on available material, systematically to differentiate human images from Fiji and from Tonga. The third sub-area of Western Polynesia, represented by a single figure from Western Samoa, has been considered above and defined as Tongan rather than Fijian in type. The more important differences between the two sub-areas claiming our attention here may be noted as follows:

FIJI	TONGA
(i) Wooden images predominant, ivory virtually absent. Tree fern used in some images of more recent manufacture.	(i) Ivory images more frequent than wooden.
(ii) Female images usual; few males. Sex marked by genitalia, with moderate or no secondary features of sex.	(ii) Near-naturalistic female images; no male images recorded. Sex marked by full development of secondary features of sex.
(iii) Great stylistic variety of images. Style a fusion of Polynesian, Melanesian and probably Micronesian elements. On the Melanesian side, relationship with the image styles of New Caledonia, Santa Cruz, and the New Hebrides.	(iii) Constant stylization of the Haapai type within a type range. <sup>11</sup> Style distinctly within the Polynesian figure tradition.
(iv) Darkened wood usual, occasionally patinated to a matt black in the Melanesian manner.	(iv) Known Tongan images in wood of light brown wood, darkened through handling and time.
(v) Images usually represent ancestors, and are incorporated into objects of utility, e.g. food-hangers, bowls, or architectural pieces. <sup>12</sup>	(v) Images appearing as god symbols. <sup>13</sup>

The following list of images from Tonga and Fiji at present known to me will give some indication of the relative frequency of type and material in each of the two regions:

Kind of Object	FIJI	Material	No. of Specimens
Oil dishes in human form		wood	7
Food-hangers with image		wood	5
Single images		wood	10
Single images		tree-fern	7
Single image		pottery	1
Necklace with eight images		ivory	1
TONGA			
Single images <sup>14</sup>		wood	4
Hooks with female images		ivory	3
Single images		ivory	7

#### Acknowledgments

I wish to thank Dr. G. H. S. Bushnell, Director of the Cambridge University Museum of Archaeology and Ethnology, for allowing me to photograph and publish these items from the Fijian collection; and Mr. Adrian Digby, Keeper of the Department of Ethnography, British Museum, for permission to publish the Western Samoan image, which appears as *Plate Mc.*

#### Notes

<sup>1</sup> Te Rangi Hiroa (Sir Peter Buck), 'Material Representatives of Tongan and Samoan Gods,' *J. Polynes. Soc.*, Vol. XLIV, p. 48; 'Additional Wooden Images from Tonga,' *J. Polynes. Soc.*, Vol. XLVI, p. 74.

<sup>1</sup> The Cambridge ivories of Haapai type were collected in Fiji. It need hardly be repeated here that although Fiji is, racially, predominantly Melanesian in character, the culture is predominantly Polynesian, mainly through long contact with Tonga and Samoa.

<sup>2</sup> G. T. Barker, *The First Voyaging of the Levuka and Butoni Clans to the Windward Islands*, Fiji Society, Transactions, Suva, 1927.

<sup>3</sup> It should be noted here that the Cambridge series of Fijian images and Tongan-type ivories have appeared as line drawings in the privately printed *Pacific Album* of James Edge-Partington and Charles Heape (Manchester, 1890-98). However, the line drawings are inadequate for any serious study, little information appears, and the work itself is now rare.

<sup>4</sup> For an example of this Hawaiian-type image see Paul S. Wengert, *Art of the South Pacific Islands* (London, 1953), Plate XCIV.

<sup>5</sup> The third hook of this type is in the collection of Marischal College, Aberdeen, and is published in the catalogue by Cyril Aldred, *Primitive Arts of the South Seas*, Edinburgh (Arts Council), n.d., Plate VIII, No. 72.

<sup>6</sup> Theodor Kleinschmidt, 'Reisen auf den Viti-Inseln, nach seinen brieflichen Mittheilungen bearbeitet.' *Journal des Museum Godeffroy*, Vol. XIV, 1879, Plate XVI. The pendant shown in this paper as Plate La-c appears on the plate as Nos. 6 and 7; Plate Ld appears as Nos. 8 and 9. Sir Arthur Gordon's note appears with other details on pages 282f.

<sup>7</sup> For an example see W. O. Oldman, *The Oldman Collection of Polynesian Artifacts*, Polynes. Soc. Mem. No. 15, Plate LXVIII.

<sup>8</sup> A. Kraemer, *Die Samoa-Inseln*, 2 vols., Stuttgart, 1902-3, Vol. II, p. 207.

<sup>10</sup> Te Rangi Hiroa, 'Material Representatives . . .', loc. cit., pp. 93-95.

<sup>11</sup> The one notable exception is in the Oldman Collection, No. 532 (now deposited in the Auckland Museum). The image is female, in a sitting position, with both legs folded to one side of the body. The plastic modelling is a unique move to naturalism in Polynesian wood sculpture. Unfortunately the specimen is much damaged. See Te Rangi Hiroa, 'Additional Wooden Images from Tonga,' loc. cit., p. 77, fig. 3.

<sup>12</sup> No attempt is made to investigate the social and religious significance of these objects here. Sir Peter Buck has provided adequate knowledge for the Tongan figures, and I understand that Mr. Karl-Erik Larsson of the Etnografiska Museet, Göteborg, is working on the cultural significance of these images within the framework of Fijian society.

<sup>13</sup> The evidence of John Williams (*Missionary Enterprises*, London, 1839, pp. 273f., 275) and that for Plate 1d adequately support this conclusion.

<sup>14</sup> Three single wooden images from Tonga are in the Oldman Collection (Polynes. Soc. Mem. No. 15, Plates XLV, XLVI and XLVII). The fourth image is a splendid large specimen in the Fuller Collection, London (see the catalogue *Traditional Art of the British Colonies* (R. Anthropol. Inst., 1949), Plate XXII, where it is illustrated with one of the Tongan ivory figures, also from the Oldman Collection and now in the British Museum). It should be noted that innumerable small anthropomorphic motifs appear on carved Tongan clubs, but these have been excluded completely from this study.

## A NOTE ON MANCALA GAMES IN NORTHERN RHODESIA\*

by

J. H. CHAPLIN

Livingstone, Northern Rhodesia

**193** In his review of Mancala games, Murray<sup>1</sup> bases his entry for Northern Rhodesia for the most part on Smith and Dale's accounts of the Ilala people.<sup>2</sup> Tracey has given the basic games played in Southern Rhodesia<sup>3</sup>; and it is perhaps of interest to place on record some of the games of this type which are played in Northern Rhodesia at the present time.

For the most part these games are played in holes in the ground, the predominating sandy soil making the excavation of cups quite easy. However, boards are in use. The example shown in fig. 1 is in the Rhodes-Livingstone Museum, and is attributed to the Balungu tribe of Mpokoso District.<sup>4</sup> The museum also has a stone on which the faint marks of cups can be seen, and others are known.

The pieces used in the game vary; stones are common as are various seeds. The seeds of the *mungongo* tree<sup>5</sup> are frequently used in the area in which this tree is found, being very durable and of a convenient size.

In an account of the history of his tribe, a Muila tells of two rival chiefs Munyama and Malumbwe. The former had lent a magician to the latter. This man had created a salt pool for Malumbwe, who had then killed him to prevent his return. The quarrel that resulted was to be resolved by what the informant describes as 'native chess.' The account continues '... they all agreed and met on the way,

and sat on a rock and made some holes for stones and they started playing and Malumbwe was won by 4 to 1.'<sup>6</sup> A further argument developed over the result and the descendants of the two groups are still said not to eat food



FIG. 1. MANCALA BOARD FROM THE BALUNGU TRIBE,  
MPOROKOSO DISTRICT

in each other's districts. The account goes on '... The Pool and the Chess are still seen, the boiling water and a plain around it still the same, the chess is on a rock, the marks of their feet, hands and stones holes are there today

\* With a text figure

in Mumbwa district at the village called Mafwele taking the road from Mumbwa to Namwala.'

The descriptions of the games that follow are in the notation developed by Murray.<sup>6</sup>

**LOZI TRIBE.** *Mulabalaba*.  $4 \times 6$  or  $4 \times 8$  holes. Two beans, sometimes three, in each hole. Play is anti-clockwise. A move starts from any loaded hole, usually in the back row and the player's left. Beans are sown singly; if the last bean falls in a hole already occupied, that hole is emptied and its total contents sown. If the last bean falls in an empty hole of the front row, and the opponent's front hole of that file is occupied, then the opponent's beans in the whole file are captured and removed from the board. When a player has only single beans in his holes, he must if possible sow a single bean into an adjacent hole that is occupied, lifting them both and sowing accordingly.

**TOKA TRIBE.** *Mulabalaba*.  $4 \times 12$  or  $4 \times 16$  holes. Two beans in each hole. Play is anti-clockwise and exactly as the Lozi game above.

**LUNDA TRIBE.** (Mwinilunga District.) *Kamona*.  $4 \times 8$  holes. Two beans in each hole. Play is anti-clockwise. Before play properly begins X takes the contents of four of Y's holes, and Y takes the same number from X. Play is the same as the Lozi game except that besides taking the contents of the opponent's file opposite the empty hole in which the player has landed, he may also remove the contents of any other two of his opponent's holes. All captures are removed from the board.

**TUMBUKA TRIBE.** *Nchumba*.  $4 \times 16$  holes. Two beans in each hole of the back row only. Play is anti-clockwise, and is the same as the Lozi game with the following variation. When X 'eats' (this is the common word used to denote capture) for the first time, he takes the opponent's file, also three other holes; when he 'eats' for the second time he takes the file and any two other holes. After third and subsequent captures he removes the contents of the file and any one other hole. At the first capture it is advantageous to remove Y's leading hole, one of his right-hand back row, and any other. While there are two beans in any hole on your side you may not move singles. The senior man begins to play, thereafter the winner of the previous round begins.

**BANTU BOTATWE (KAONDE ILA) TRIBE.** *Nakahili*.  $4 \times 8$  holes. Two beans in each hole except the front left-hand hole which is left empty, and the hole on the right of this which has one bean only. Play is anti-clockwise, and is the same as the Lozi game with the following variation. When X 'eats' for the first time he removes the contents of Y's file plus the contents of two other holes. Y does similarly when he first 'eats.' On the second and subsequent captures he removes the opponent's file plus one other hole. If all the holes have singles except one which has two, this is called *mutuwi* (the head) and must be moved.

**BANTU BOTATWE (KAONDE ILA) TRIBE.** *Kahwenga* (this is a nickname for the hyena, referring to its habit of snatching goats which have been tied up).  $4 \times 8$  holes. Two beans in each of the back row holes, two beans in the front right-hand hole, one bean in the hole next to this, the remainder of the front row empty. Play is anti-clockwise, and is the same as the Lozi game with the following variation. If you end your lap in an empty hole and the hole ahead of it (i.e. to the left of it) has two or more beans in it, then you have another lap; if the following hole has only one bean in it, this advantage cannot be taken. The movement of the *mutuwi* mentioned in the previous Bantu Botatwe game also applies to this.

**TONGA TRIBE.** *Chisolo II* (I suspect that there is a different 'old people's' name for this game but have been unable to find it).  $4 \times 5$  holes. Three beans in each hole. Play is anti-clockwise. Play is the same as in the Lozi game with the variation that besides capturing the contents of the opponent's file, the player may also take and discard the contents of any one other hole.

**TONGA TRIBE.** *Chisolo II* (the note above concerning the name applies here also).  $4 \times 5$  holes. Three beans in each hole. The senior man begins the first game, thereafter the loser of the previous game begins. Play usually begins on the right-hand side of the back row. Beans are sown singly; if the last is sown into an already occupied front hole, the opponent's file opposite to this may be captured provided he has beans in the front hole of the file. To land in an empty hole finishes the lap and you cannot 'eat.' The captured beans are resown into the capturer's rows, the resowing beginning at the hole next to the one the filling of which enabled the capture to be made. If sowing results in the last bean landing in a full hole and no capture is possible as the opponent's file is empty, then the contents of the hole are removed and resown.

As this is the first re-entry game described in this note a sample game is given below using the score defined by Murray:

*Y3f : 3D3G Y(6) Y4e  
X3B X4E : 5g4d X(9f) : 4c5h X(9l) : 4b5i X(9H) : 4a4j ENDS*

This is a very brief game; one of 60 laps has been recorded, and 30 is probably the average.

**BEMBA TRIBE.** *Mwambulula*.  $4 \times 8$  holes. Two beans in each hole; the front rows are at once cleared to make a store. Play is anti-clockwise. If a player lands in a full hole he may 'eat' his opponent's opposite file provided that the front hole has beans. If he is unable to 'eat,' the contents of the hole are resown. The captured beans are re-entered, the sowing beginning at the hole from which the 'eating' move commenced. At any time during the game when your own beans are running short you may empty your store and add one bean to each of your holes.

**BEMBA TRIBE.** *Isolo I*.  $2 \times 8$  holes. Two beans in each hole plus a store of indefinite size. The game is played in two parts, which I shall call 'fore-play' and 'main game.' The fore-play begins with a player taking one of the store beans (called *nkonto*), this is added to a hole and the entire contents are removed and sown anti-clockwise beginning at the left-hand hole of his row. If the final bean is sown in an empty hole, the lap is finished and the player 'sleeps.' If it lands in an occupied hole he can 'eat' his opponent's beans. Resowing of the captures follows certain rules. In the following, Y is assumed to be capturing men from X.

- (1) If the beans captured occupy holes C, D, E, F, they are resown beginning at a.
- (2) If there are two or more beans in holes A, B, G, H, they are resown beginning at a.
- (3) If there is a single bean in B, on capture it is added to h; if A is also occupied the contents are captured, added to h and the total contents of h resown beginning at a. (These single beans are called *mwipinda*).
- (4) If there is a single bean in G, on capture it is added to a; if H is occupied the contents are captured, added to a, and the total contents of a are resown beginning at b.
- (5) If any of the above resowings results in a further capture, the captured beans are resown beginning at a.
- (6) If a player in sowing, or resowing, ends in a hole already occupied, but the opposite hole is empty, then he takes the contents of his own hole and continues sowing. If a capture now results, the captured beans are resown beginning at the hole next to that emptied to begin the lap, subject to the overriding provisions of (3) and (4) above.
- (7) If the store bean is added to g, the contents of g are resown beginning at h, not a.

When the fore-play results in a decision, the loser sets two beans in each hole. The winner arranges his beans in the way he thinks most to his advantage with no limitations as to number of beans used or holes occupied. This is called *kucheleka*. The main game then continues in accordance with the rules outlined above.

**BEMBA TRIBE.** *Isolo II*.  $4 \times 8$  holes. (I am uncertain whether this is an authentic tribal game; it may be a town simplification of the previous game.) Play is anti-clockwise. Two beans are sown in

each hole to see that the correct number are being used. Then each man fills his holes as he likes, with a maximum of 12 beans in any one hole, and generally filling the back rows rather than the front ones. The board being set a player begins by taking the contents of any hole, sowing singly. If the last bean lands in an occupied front hole and there are also beans in the back row of this file, then he can 'eat' his opponent's opposite file, provided there are beans in the front row. The captures are resown beginning at the hole next to that which was emptied to begin the lap, and so on with further captures. If the last bean lands in an empty hole, the lap ends.

I am unable to give notes on any other mancala games at present, but there are two other games which may be worth recording.

A game which is popular in the Livingstone district is called *mulabalaba* by the Lozi, which is the same word used to describe the mancala game. The board is marked out on cardboard or a table, and is a pattern of three squares one inside the other with the corners and midpoints of the sides joined by straight lines. This is a typical three-in-a-line board (Murray shows this<sup>8</sup>). Maize seeds, whites and purples, are often used as counters. Each man has 12 and these are entered alternately. When either makes a line of three he can remove any one of his opponent's counters. When all the pieces are entered the main game begins; counters being moved along the straight lines to form lines of three, with resulting captures. Jumping is not allowed except that a player having a counter at an outside corner may move it wherever he likes. The loser begins the next round.

To complete this note a competitive game of skill may be mentioned. This is the Tonga game *kuyata*. Twelve *mungongo* seeds are put in a hole in the ground and the player squats by it. One seed is then taken out and thrown up; while it is in the air the player watches it and while

ready to catch it with one hand must grab as many seeds as he can out of the hole with the other. If the thrown seed is caught all the withdrawn seeds are returned but one. So the game continues, one of the withdrawn seeds being kept out until all are removed. If the player fails to catch the thrown seed then all the gains that have been made have to be returned to the hole and the game begins again. Prestige for speed is the reward. I have only seen it played by children, usually girls.

#### Acknowledgments

I am grateful to the Curator of the Rhodes-Livingstone Museum, Dr. J. Desmond Clark, O.B.E., for allowing me to photograph and measure the boards in the Museum collection.

Many African friends have helped in the preparation of this paper, but I must especially mention Mr. Raphael Chitalima, Mr. Simon Micello and Mr. Maxwell Shanyimbo, for their patient help.

#### Notes

<sup>1</sup> H. J. R. Murray, *A History of Board-Games Other Than Chess*, Oxford, 1952.

<sup>2</sup> E. W. Smith and A. M. Dale, *The Ilia-speaking Peoples of Northern Rhodesia*, London, 1920.

<sup>3</sup> H. Tracey, 'The rules of the native game Tsoro,' N.A.D.A. No. 9, pp. 33f., Salisbury, Southern Rhodesia, 1931. This is a description of the game using an odd number of holes which is an unusual feature. See also Sanderson in *J. R. Anthropol. Inst.*, Vol. XLIII (1913), pp. 726f.

<sup>4</sup> The dimensions of the board, excluding the store, are 55×30 centimetres. Another example consists of two rows of 8 holes with a store; this is 85×15 centimetres overall. It is made by the Bisa tribe for their local game of *iisolo* of which I have no note.

<sup>5</sup> The Silozi name for *Ricinodendron rautanenii*. The seeds are ovoid, 2·5 centimetres long, 2·0 centimetres in diameter. Mainly found in the Kalahari Sand country.

<sup>6</sup> Murray, *op. cit.*, pp. 164f.

<sup>7</sup> *Ibid.*, p. 214.

<sup>8</sup> *Ibid.*, fig. 18, type G.

## SHORTER NOTES

**A Bushongo Cup in the Musée de l'Homme.** By *Mme Jacqueline Delange, Département d'Afrique noire, Musée de l'Homme, Paris*; translated by Miss M. A. Bennet-Clark.  
With two text figures

In 1954, the Musée de l'Homme acquired a small collection of pieces of African art, the gift of M. André Lefèvre. This consisted chiefly of Bushongo (Bakuba) pieces, especially of cups and boxes, the forms and ornamental combinations of which figure frequently in writings on African art.

One of these cups deserves particular attention. Its cylindrical form; the preference shown for the three-strand twist motif *imbolo* (on the outer wall and the outside base of the cup, where the closed *imbolo* approximates to a cross); the overall decorative treatment of the surface with interlacing diagonal incisions which form a background and with secondary motifs (chevrons and obtuse-angled twists, *dembo* or the toe pattern, all very flexibly treated in our example); the handle carved in the form of a loose knot, having on its upper part a face recalling the sun motif *phila*; the quality of the wood, with its brown patina and reddish highlights—all these form an ensemble well known to be characteristic of Bushongo art. However, in the centre of this cup, fitted into a hole 1·4 centimetres in diameter, is a stopper which slightly recalls the anthropomorphic handles of Bakongo (Bawongo) ceremonial adzes<sup>1</sup> though our stopper, 8·9 centimetres

high, is more finely worked. Its section is cylindrical only up to the head, where the wood is hollowed out, causing the skull and coiffure to project backwards, in a manner reminiscent of the shape of Mangbetu women's heads. This similarity is strengthened by the hieratic cast of the features, with the almond-shaped mouth in relief, the slight definition of the cheekbones, the eye orbits and the nose which are placed high up near the top and very worn by the polishing of thumbs. The ears, which are small and sharply defined, underline the character of the head, which is supported on a long annulated neck. The stopper is loose, but the small gap between its end and the edge of the hole may lead one to think of a small wad (perhaps of vegetable fibre), which in securing the stopper would give it a different function. It should be noted that the external base of the stopper is cross-hatched with fine lines that fit perfectly into the general pattern on the base of the cup without any interruption in the design. There is no fault in the wood to which the existence of this hole, and of the head, could be attributed.

We do not know of any comparable piece, either in the specialist literature or in exhibition catalogues. The only known reference to our piece is in the catalogue by M. Charles Ratton of a sale of objects from the A. Lefèvre collection on 6th May, 1931, which marks its brief transit at the Hôtel Drouot (No. 98, illustrated on Plate III). The bidding did not reach 2000 francs!



FIG. 1. A BUSHONGO CUP IN THE MUSÉE DE L'HOMME

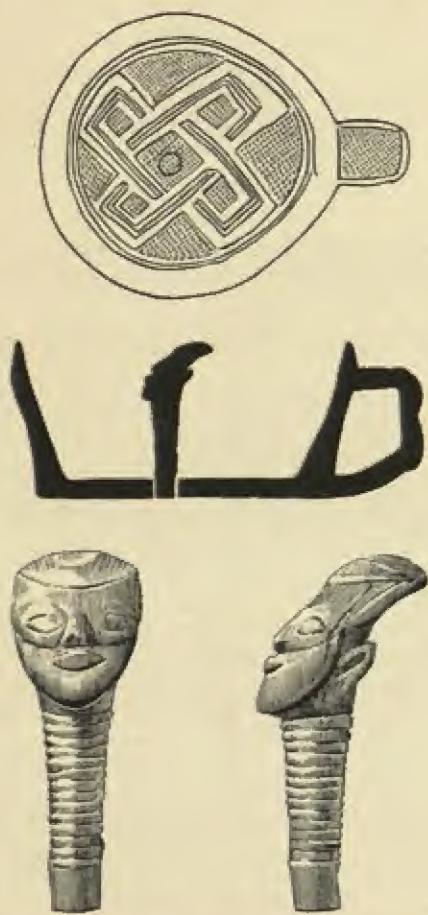


FIG. 2. DETAILS OF THE BUSHONGO CUP

The Musée Royal du Congo Belge has about 100 Bushongo cups, but not one is perforated or has an anthropomorphic stopper. The only piece that Professor Olbrechts mentions is a cup in the form of a truncated cone, very much carved over, from the middle of which rises a vertical tube carved in one piece with the whole.

He also informs us that Dr. A. Maesen, who has recently returned from the Kasai region, has seen nothing like it.

Thus we have a unique piece, the function of which remains unexplained. It may, however, be linked up with certain other Bushongo carvings, namely the handles of ceremonial adzes,<sup>1</sup> and the above mentioned cup, formerly in the collection of Iritz Van den Berghe, which is published by Dr. Olbrechts.<sup>2</sup> We would add to these the cup<sup>3</sup> illustrated in Torday's section on Bushongo art, the general conception of which is identical with ours (hole and stopper excepted); and finally, the lid, in the form of a truncated pyramid, of a box for *tukula* (camwood) in the British Museum, also illustrated by Torday.<sup>4</sup> This lid is perforated in the centre, and shoulders, arms and hands are carved on each side of the hole, suggesting that a head and neck were meant to be inserted.

#### Notes

<sup>1</sup> E. Torday and T. A. Joyce, 'Notes Ethnographiques sur les peuples communément appelés Bakuba . . . : Les Bushongo,' *Annales du Musée du Congo Belge* (1910), p. 202, fig. 292.

<sup>2</sup> F. M. Olbrechts, *Plastiek van Kongo*, 1948, Plate XV, No. 73.

<sup>3</sup> Torday and Joyce, *op. cit.*, p. 204, fig. 296c.

<sup>4</sup> *Ibid.*, Plate XXV, No. 3.

#### The Centenary of Neandertal Man: Part II. By Bernard Campbell. With two text figures

**195** At the beginning of 1864, the Neandertal controversy centred on King's important paper in the *Quarterly Journal of Science*.<sup>1</sup> Not only had King suggested a specific status for the fossil, but he had gone further: he had written 'so closely does the fossil cranium resemble that of the chimpanzee, as to lead one to doubt the propriety of *generically* placing it with man.' But he wisely added: 'to advocate this view in the absence of the facial and basal bones would be clearly overstepping the limits of inductive reasoning.' He did finally overstep such limits, however, when he argued that Neandertal man must, with the Chimpanzee, have been incapable of 'moral and theistic conceptions.' In the following issue of the *Quarterly*, for April, Turner, the Edinburgh anatomist, urged caution in making such far-reaching deductions on the basis of an isolated specimen.<sup>2</sup> Huxley re-echoed his wise remarks, and with Wallace considered the skull to belong to a member of a 'savage' race in a low state of development. Those who could not accept the theory of evolution, however, refused to acknowledge any but a pathological theory to account for the strange skull. Barnard Davis ingeniously explained it as the result of the early synostosis of the calvarial bones in the temporal region; he considered his theory to be proof of the abnormality of the skull.<sup>3</sup> In Germany anatomists such as Welcker and Vogt supported the pathological theory, and Wagner suggested that the skull was that of a modern Dutchman!

The Gibraltar skull, which Busk showed at the Bath meeting of the British Association in September, 1864, should have been enough to convince most anatomists of the existence of a distinct prehistoric form of man. In 1865 there followed the discovery of the famous mandible of La Naulette by Dupont, an eminent Belgian geologist. A few years later, more fossils were found in eastern Europe, some clearly of the Neandertal group, such as Šipka (1881) and Podbaba (1883)—the second has since been lost—and others of an early 'sapient' type. In 1886 two relatively complete skeletons were discovered at Spy, near Namur in Belgium, the first Neandertal remains found with skull and mandible *in situ*. They were excavated by de Puydt and Lohest under ideal scientific conditions. The geological horizon was well established, and a Middle Pleistocene faunal context and a Mousterian industry were clearly identified. Fraipont and Lohest published an excellent

monograph on the find the next year.<sup>4</sup> Other discoveries now followed fast, in particular that of the Malmaud mandible (1889) and the important group of fossils from Krapina (1899).

The accumulation of palaeontological evidence was considerable. Anthropological studies became directed towards cataloguing and defining the different races of living and extinct man; nomenclature assumed a position of some importance. One of the most remarkable publications of this period was that of de Quatrefages and Hamy in 1882.<sup>5</sup> These authors took the obscurely known fossil from Cannstatt as the prototype of their '*première race fossile*' This particular cranial fragment was discovered as early as 1700, but was not described in any detail until 1835; both its provenance and its age are now considered doubtful. De Quatrefages and Hamy surprisingly enough seem to have associated its early discovery with an extreme age, for they classified Neandertal man as a member of this Cannstatt race, although Schwalbe has since clearly shown the fragment to belong to *Homo sapiens*.<sup>6</sup>

As the number of fossils increased the group became more clearly defined, and after the Spy discoveries, Neandertal anatomy became relatively well known. As a result, anatomists began to recognize that a distinct status for the group was not unreasonable. Haeckel had suggested the names *Proanthropus atavus* or *Homo primigenius* for a Pleistocene early man, and in his book of 1895 he considered the fossils from Neandertal and Spy to fall into this class. The names, however, are not valid designations for these fossils according to the International Rules of Zoological Nomenclature. Wilser, in his enthusiasm for classification, introduced the name *Homo europaeus* for European man, and added for Neandertal man the subspecific term *primigenius* suggested by Haeckel.<sup>8</sup> He thus separated Neandertal man subspecifically from modern Europeans. The next year Lapouge placed Neandertal man in the newly created genus *Pithecanthropus* Dubois of 1894.<sup>9</sup> He considered Neandertal man to be a regional variation of *Pithecanthropus* and believed both to lie very close to the direct ancestry of *Homo sapiens*.

Towards the turn of the century, Gustav Schwalbe wrote extensively on the problems of fossil man. He recognized the specific status of the Neandertal group, and, with Cope in the United States, he followed King's nomenclature. In his publication of 1901, he gave a full review of the level of opinion of the leading anatomists and anthropologists at the time.<sup>10</sup> In 1902, however, Schwalbe seemed to favour an entirely new nomenclature for the genus *Homo*,<sup>11</sup> and suggested that 'the name *Homo hodiernus* would most easily correspond to what we call modern man, in contradistinction to *Homo Neanderthalensis* for which the name *Homo primigenius* would commend itself.' Schwalbe finally adopted this nomenclature in his paper of 1903; accepting the term *primigenius* as more suitable in view of the primitive characteristics of the group.<sup>12</sup> His usage has been followed by most anthropologists in Germany until quite recently, and is frequently seen even today. In Vallois and Movius's published catalogue of human fossils, however, *Homo neanderthalensis* King is accepted as correct by Gieselet.<sup>13</sup>

After the turn of the century, the anatomical study of fossil man was greatly developed. The stimulus of Dubois's discovery of *Pithecanthropus* in Java had caused the greatest anatomists of the day to turn their attention to the problem. By 1908, when the skeleton of La Chapelle-aux-Saints was discovered, Boule was able to write a monograph upon the fossil which has stood as a prime example of its kind ever since.<sup>14</sup> At this time, the straight path of evolution from Neandertal to modern man seemed clear enough to most biologists. The theory of particulate evolution, which would allow the brain to evolve before the body, accounted for the apparently disharmonious combination of the Neandertal skeleton with a full-sized brain. Doubts on this theory of direct

descent had been expressed, but with the discoveries of *Pithecanthropus* in 1891 and of the very primitive Mauer mandible in 1907, it gained ground. The notorious cranium and mandible of Piltdown, announced in 1912, presented the first major difficulties in the theory. Here, apparently, was a middle Pleistocene skull almost indistinguishable from *Homo sapiens*: but its astonishing association with an ape-like jaw resulted in its being placed by most authors on a side branch outside the direct line of human evolution. But the cranium presented a problem, even if the jaw did not belong to it! In his Huxley Memorial Lecture of 1927,<sup>15</sup> entitled 'The Neandertal Phase of Man,' Aleš Hrdlicka upheld the hypothesis implicit in his diagram of human descent (fig. 1), and

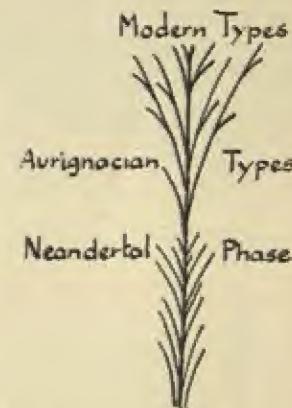


FIG. 1. DIAGRAM OF HUMAN DESCENT  
(after Hrdlicka)

this theory was generally accepted at the time. Those who believed in the greater antiquity of *Homo sapiens* found Piltdown Man little to their comfort. For their justification, they had to await the important discovery of the Swanscombe calvarial bones in 1935, which, being in all major respects like *Homo sapiens*, raised again the whole problem of the place of Neandertal man in human history. The bones seemed of undoubted authenticity and age, and came from the penultimate interglacial, with an Acheulian industry. The fluorine test and the recent discovery of the second parietal bone have confirmed this conclusion.

As the Gibraltar skull served to confirm the existence of Neandertal man, so the fossils of Fontéchevade vindicated the theories built upon the evidence of the lone Swanscombe man. Neandertal man was removed from the direct line of human descent, and became an abortive side branch: the origin of *Homo sapiens* was put back to nearly half a million years ago. The new situation was well expounded by Vallois in 1954.<sup>16</sup>

But these discoveries of Swanscombe and Fontéchevade were not the only additions to the human family. In Europe, since 1945, remains of at least ten new specimens have been added to the list of Neandertal fossils. An important summary of Neandertal anatomy has also recently been published.<sup>17</sup> Further east, the less highly evolved group, which McCown and Keith called *Homo palestinus*, has been enlarged by discoveries at Jebel Kafzeh (1935), Teshik-Tash (1938), and Staroselje (1953). In addition to that from Mauer, mandibles of a much more primitive type have been found at Montmaurin in the Pyrenees and at Ternifine in North Africa. In South Africa, the Australopithecine group has become one of the best represented in primate palaeontology.

It has become evident from these new discoveries, especially those from Europe and its borders, that the history of Neandertal man is a repetition of a process that biologists have been led to expect to occur in the normal processes of animal and plant

evolution. It is now clear that the evolution of a species group, such as *Homo sapiens*, is the outcome of a complex process of evolutionary deployment, geographical, anatomical, and, in this case, cultural, which results in a population passing into isolation and undergoing adaptation to a new environment. Research in animal evolution leads us to expect a centre of evolution to be a complex of evolving populations, each placed in varying degrees of isolation from its neighbours; those which pass beyond the perimeter of such a centre may as often become the abortive side branches of the evolving stock as develop into a new line of ultimate importance. While overspecialization may spell the fate of such an isolated group in a time of environmental change, underspecialization can result in its inferiority in face of subsequent competition.

In the more recent stages of the evolution of the Hominidae, it now seems that Europe played the part of an adaptive zone into which evolving populations passed from a centre further east. The geographical barrier of the Atlantic halted their spread, and

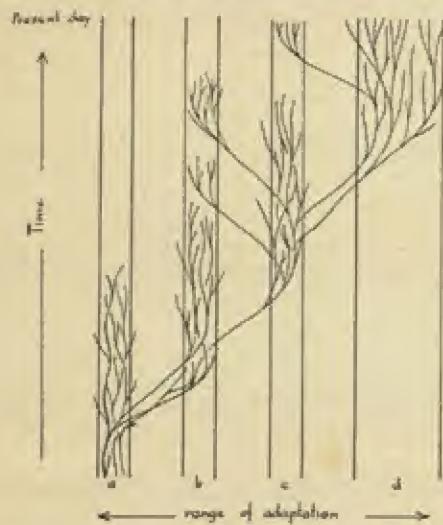


FIG. 2. EVOLUTION OF A GROUP THROUGH ADAPTIVE ZONES

The diagram represents a group such as the Hominidae evolving as demes through different adaptive zones (a, b, c, d) which are often, though not invariably or directly, related to different geographical and climatic zones.

their new environment demanded a degree of local specialization. After a period, it seems likely that less specialized and more vigorous groups again advanced from the east to replace, or hybridize with, those then in occupation of the fertile lands of western Europe, lands which had never been ice-bound nor very arid. With this in mind, it is unreasonable to expect to find in Europe any evidence of a direct line of descent for our own species. Instead we can expect fossil groups, which may follow closely after one another, to display anatomical differences of a subspecific or even a specific degree, and to show little in the way of a clear evolutionary pattern of development. The inter-

mediate stages of development will be found to lie further east (fig. 2).

Thus today Neandertal man is seen as one of these abortive side branches, highly successful for a time, and then doomed to extinction. The attention of human palaeontologists, after a century of devotion to this famous species, has now turned back to the problem of the men who occupied Europe in pre-Neandertal times and to the wider and infinitely more complex problem of the origin of *Homo sapiens*.

#### Notes

- <sup>1</sup> W. King, 'The Reputed Fossil Man of Neandertal,' *Quart. J. Sci.*, Vol. I (1864), pp. 88-97.
- <sup>2</sup> W. Turner, 'The Fossil Skull Controversy,' *Quart. J. Sci.*, Vol. I (1864), pp. 250-8.
- <sup>3</sup> J. B. Davis, 'The Neandertal Skull: Its Peculiar Conformation Explained Anatomically,' *Mem. Anthropol. Soc.*, Vol. I (1865), pp. 281-95.
- <sup>4</sup> J. Fraipont and M. Lohest, 'Recherches ethnographiques sur les ossements humains découverts dans les dépôts d'une grotte quaternaire à Spy,' *Arch. Biol.*, Vol. VII (1887), pp. 587-757.
- <sup>5</sup> A. de Quatrefages and E. T. Hamy, *Crania Ethnica*, Paris, 1882.
- <sup>6</sup> G. Schwalbe, 'Das Schädelfragment von Cannstatt: Studien zur Vorgeschichte des Menschen III,' *Z. Morph. Anthropol.*, Vol. I (1906), pp. 183-228.
- <sup>7</sup> E. Haeckel, *Systematische Phylogenie der Wirbeltiere*, Vol. III, Berlin, 1895, pp. 613-50.
- <sup>8</sup> L. Wilser, 'Menschenrassen und Weltgeschichte,' *Naturw. Wschr.*, Vol. XIII (1898), pp. 1-8.
- <sup>9</sup> G. Lapouge, *L'Aryen: son rôle sociale*, Paris, 1899.
- <sup>10</sup> G. Schwalbe, 'Der Neandertalschädel,' *Bonn. Jb.*, Vol. CVI (1901), pp. 1-72.
- <sup>11</sup> Id., 'Neanderthalschädel und Friesenschädel,' *Globus*, Vol. LXXXI (1902), pp. 156-74.
- <sup>12</sup> Id., *Die Vorgeschichte des Menschen*, Braunschweig, 1904.
- <sup>13</sup> Henri V. Vallois and Hallam L. Movius, Jr., 'Catalogue des Hommes fossiles,' *Proc. XIX Int. Geol. Congr.* (Algiers), Vol. V (1953), p. 77.
- <sup>14</sup> M. Boule, 'L'Homme fossile de La Chapelle-aux-Saints,' *Ann. Paléont.*, Vol. VI (1911), pp. 106-72; Vol. VII (1912), pp. 21-192; Vol. VIII (1913), pp. 1-70.
- <sup>15</sup> Aleš Hrdlička, 'The Neandertal Phase of Man,' *J. R. Anthropol. Inst.*, Vol. LVII (1927), pp. 249-74.
- <sup>16</sup> Henri V. Vallois, 'Neandertals and *Præsapiens*,' *J. R. Anthropol. Inst.*, Vol. LXXXIV (1954), pp. 111-30.
- <sup>17</sup> É. Patte, *Les Néandertaliens*, Paris, 1936.

#### Horniman Museum Lectures, Spring, 1957

The following are among the lectures of anthropological interest to be given at the Horniman Museum, London, S.E.23, at 3.30 p.m. on Saturdays: 5 January, 'The Music of the North American Indians,' with recordings, by Dr. Bruno Nettl; 26 January, 'Clay and Skill: The Potter's Craft,' by P. O'Malley, Esq.; 2 February, 'Early Man's Use of Fire,' by Dr. K. P. Oakley; 23 February, 'A Swedish Fishing Community,' by Mr. K.-E. Larsson; 2 March, 'Celtic Folklore and Scenery,' by Mrs. E. Ettlinger; 9 March, 'Music of Africa,' with recordings, by Dr. J. Kunst.

## REVIEWS

### AMERICA

**The Calendar of Tiahuanaco.** By H. S. Bellamy and P. Allan.

197

London (Faber), 1956. Pp. 440. Price £2 10s.

One of the authors published a book called *Built before the Flood* some years ago. He says that this is not merely a new edition of it, but it is based on the same ludicrous assumptions, and all that is new in it consists of similar mathematical and

astronomical fancies squeezed out of yet more of the details carved on the Tiahuanaco monolithic gateway. The whole edifice is founded on the unwarranted assumption that the carvings are a calendar, and on an astronomical theory which, the book says, 'astronomers have so far ignored.' They will doubtless continue to do so.

The actual date of the monument must fall somewhere in the first

millennium A.D., but the authors say that the 'calendar' cannot date from later than 25,000 B.C. They do not appear to realize that this date is well before the end of the Upper Palaeolithic, but even if they did, it does not appear that little difficulties of that sort would trouble them at all, for they observe blandly that 'archaeologists must revise their chronology'!

G. H. S. BUSHNELL

**198** *The Art of Ancient Mexico.* 109 photographs by Irmgard Groth-Kimball; text and notes by Franz Feuchtwanger, London and New York (Thames & Hudson), 1954. Pp. 125. Price £2 2s.

The exhibition of Mexican Art which was shown successively in 1951 in Paris, Stockholm and London drew the attention of a large public to the art of this part of the world, which until then was only known to a relatively small group of specialized scientists and collectors. In its wake several volumes have appeared which are chiefly concerned with Precolumbian Art. Among them the work of Franz Feuchtwanger and Irmgard Groth-Kimball is certainly one of the most rewarding.

The very well reproduced photographs give a good idea of the monuments of architecture and sculpture. Of course the 109 plates do not exhaust the vast field. The small objects in precious stone are very thinly represented, and the painted manuscripts or codices are completely left out. Not one photograph tells us anything about the art of the North of Mexico. So we might say that the authors have limited themselves to the task of giving us a picture of Meso-American Mexico. A few coloured plates complete the presentation very well. The works used come for the most part from Mexican collections, though some outstanding pieces are in the British Museum.

Feuchtwanger, who takes responsibility for the text, tries successfully to give a general view of the principal civilizations in a few pages. He takes into account the archaeological findings of the last few years. He is himself a well-known collector and has specialized in the archaic civilizations, but he does not content himself with a description of the different styles, trying on the contrary to analyse the symbolism of certain objects. A chronological chart at the end of the text will be of use chiefly to the non-specialized reader.

The description of the photographs is conscientious, and gives the measurements of the different objects.

The book is a good introduction to the Precolumbian art of Mexico and it will be welcomed by art-lovers and collectors. Editions of this book in French, Spanish and German have already appeared.

HENRI LEHMANN

**199** *Hopi Ethics: A Theoretical Analysis.* By Richard B. Brandt. U. of Chicago P. (U.K. agents, C.U.P.), 1954. Pp. x, 398. Price £2 16s. 6d.

One can only view this thorough, detailed and ponderous book as an example of what modern technically-minded philosophers can do to what is intrinsically interesting substantive material. The richness and complexity of Hopi life as here presented in several hundred pages of descriptive example and illustration remains inert and static. By a curious transvaluation of the historic basis of interest in primitive society, the Hopi and their society are now transformed into a set of logical constructs.

The critical problems of ethics, we are told, consist 'of the analysis of ethical terms and the epistemology of ethical knowledge.' The ethics of primitives are of interest to the author, then, only in so far as they provide the philosopher with counter-instances that can invalidate his laws and theories as developed in western society: the primitives serve as a 'stimulus to philosophical progress.' Philo-

sophical progress consists of defining terms, explicating concepts, and devising new categories. By reducing ethics to the purely professional problems of Western philosophy as now dominantly constituted, the social meaning and significance of ethical attitudes lose all relationship to the problems of Hopi life and society.

The basic tone of the book is illustrated by its treatment of what is still a very important and vexing problem for both anthropology and philosophy—ethical relativism. The problem is here treated as a logical problem. Brandt asks, 'is it possible for one person to say something is wrong and another person to say it is not wrong?' and answers by saying that it 'depends wholly on the use of "wrong" in English. For this reason, facts about the ethical opinions, attitudes, and even the linguistic usages of primitive people are quite irrelevant.' Logically, of course, it is true that the study of primitives will not yield solutions to the issues and problems that arise from the contemporary mood of ethical and cultural relativism.

It is equally true, however, that one of the central problems of modern anthropology and philosophy derives precisely from the alienation of modern man as well as his primitive cohorts from ethically imperative behaviour and convictions. The social and psychological processes by which this occurs and the fact of its occurrence are meaningful philosophical problems that only become irrelevant to philosophy when it abdicates its historic interest and role in Western society.

ARTHUR J. VIDICH

**200** *Cultural Differences and Medical Care: The Case of the Spanish-speaking People of the Southwest.* By Lyle Saunders. New York (Russell Sage Foundation), 1954. Pp. 318. Price \$4.50

This book has a twofold motive and interest. A description of the Spanish-speaking population, rural and semi-urbanized, of the Southwestern States and of their attitude to health, sickness and medical treatment, is subordinated to the practical aim of showing how a more intelligent, more adaptable and less self-satisfied public health service can bridge the gap between the Spanish and 'Anglo' cultures. The problems which Dr. Lyle Saunders discusses are not peculiar to the region in which he has studied them, and his work may be of more than local usefulness.

BARBARAAITKEN

**201** *Pelican in the Wilderness.* By F. Fraser Darling. London (Allen & Unwin), 1956. Pp. 380. Price £1 5s.

We have heard much of soil erosion due to overgrazing in Africa, but it is a surprise to learn that this is a serious problem in many parts of the U.S.A., especially in the west and south. The author, on the invitation of the Rockefeller Foundation, visited many of the affected States, and conferred with those engaged in conservation, etc. It seems that vast areas are becoming barren owing to misuse by small farmers, and because 'homesteading' is traditionally sacrosanct it is very difficult for the authorities to do anything about it. The homesteaders just exist till they have exhausted the soil and then have to go. Even in New York State there is much abandoned farmland, and where things are better the standard of living is low. 'Going through some of the remoter sections of Wisconsin, my hosts remarked how prosperous things were compared with the nineteen-thirties. "There's scarcely a house now but has its windows glazed."

Another problem is that of overgrazing by state-preserved deer in the Rockies, where sense and sentiment are in conflict.

The author also visited Mexico and Alaska, studying similar problems, and the book, though written in a somewhat slipshod style, and overloaded with details of travel and food, contains much of interest about these lands.

RAGLAN

## ASIA

**202** *Report on the Excavations at Nasik and Jorwe, 1950-51.* By H. D. Sankalia and S. B. Dev. Poona (Deccan Coll.), 1955. Pp. xx, 178

There were four periods of occupation on the mound at Nasik on the upper Godavari: I, Chalcolithic (c. 1500-500 B.C.); II, Early Historic (200 B.C.-A.D. 50); III, Roman Contact (A.D. 50-200); IV, Early Muslim and subsequent (A.D. 1400-1875). The Jorwe

site was only occupied in the first of these periods. Unfortunately, the excavations have not filled the important gaps between periods I and II and between the Palaeolithic and the Chalcolithic. But they have brought nearer a complete stratigraphy of the Deccan, and have cast much fresh light on the cultures revealed, notably supplementing the work at Brahmagiri. From period II has come a very rich collection of Northern Black Polished and contemporary

pottery, some terra-cotta figurines, and relics of a remarkable bone-point factory; while the chalcolithic strata of Nasik and of Jorwe have yielded, in addition to six flat bronze axes of the Indus type and a copper bangle, enough material to characterize firmly the painted pottery and flint industries. The discovery of the 'Pressigny' technique of flint blade manufacture at Jorwe is of the first importance. May it be suggested that the puzzling ridge on reject cores (p. 152) is not a guiding ridge but the ridge formed at the 'back' of the core by the convergence of two series of rough facets, which were intended to provide platforms from which the two more regular series of transverse flakes were struck to form the first guiding ridge on the 'face' of the core?

The flints, as well as the objects of glass, stone, shell, pottery and metal, are described and illustrated with clarity and precision, and the maps, sections, figures and plates are of the first quality. The scientific decipherment of the clues to the bronze and copper techniques will be particularly appreciated; perhaps it may later be possible to subject the iron objects to a similar analysis? This publication has amply justified the enterprise of the Deccan College and the University of Poona. It is good news that many other promising sites have been discovered in the neighbourhood, and that work has already begun at one of these, Nevada, in the district of Ahmednagar.

W. C. BRICE

**They Wrote on Clay.** By Edward Chiera. Univ. Chicago P. (U.K. agents, C.U.P.), 1956. Pp. xv, 235. Price 8s. 6d.

**203** This is the eighth impression of a popular account, first published in 1938, of life in Babylonia and Assyria as revealed by archaeology and the study of the clay tablets. There are also references to the neighbouring civilizations, including those of the Hittites and the Indus Valley. It is well and clearly written, well illustrated and produced, and seems admirably calculated to introduce the discoveries of Mesopotamian archaeology to the general public.

RAGLAN

**Science and Civilization in China. Vol. I.** By Joseph Needham, F.R.S. Cambridge (C.U.P.), 1954. Pp. xxxviii, 318, maps, figures, plates. Price £2 12s. 6d.

**204** This, the first of seven volumes, provides the introductory Orientations for Dr. Needham's long-planned study of Chinese science and civilization. Detailed criticism must await the completion of this vast undertaking, for the present volume cannot fairly be treated as an isolate. In it Dr. Needham sets the scene for his subsequent treatment of the history of scientific thought, mathematics, astronomy, geology, physics, chemistry, biology and the applied sciences and technology. The importance of the project is hard to overstate. After some centuries of sinology, China remains Cathay for most westerners: Chou bronzes, Confucius, Sung painting, Ming porcelain, Chin P'ing Mei, and those poets upon whom Dr. Waley has lavished so much skill and affection. With the publication of his first volume Dr. Needham has started upon the task of amplifying and correcting this connoisseur's-eye view.

The work contains, after an ample discussion of sources, language, etc., a geographical introduction and a long historical summary to serve as a framework for the whole. There are some 50 pages of bibliography of Chinese, Japanese and western works, and an admirable index. Typical of the *aperçu* which occur is Dr. Needham's observation that the system of *fan-hieh* developed by Chinese linguists is important in the history of tabular notation and coordinate geometry. Incidentally, the practice throughout the work is to use a system of transcription based upon the familiar Wade-Giles, with the aspirate replaced by an *h* (e.g. *chhieh* for *ch'ieh*), characters being relegated to footnotes. Inevitably these introductory sections suffer from compression and to some extent from reliance upon authorities whose views are still a matter for discussion. It is to be doubted whether many specialists would wholly accept the scheme implied by the map of human migrations at fig. 2, based upon Griffith Taylor. Again, Andersson's dating of the later phases of the Neolithic in North China are by no means universally accepted.

But the crux of the present volume is the long portion, some hundred pages, entitled 'Conditions of Travel of Scientific Ideas and

Techniques between China and Europe.' Upon this chapter depends the answer to the question which Dr. Needham poses in his preface: What exactly did the Chinese contribute in the various historical periods, ancient and medieval, to the development of science, scientific thought and technology? For, as the author points out, after the arrival of the Jesuits, 'Chinese science gradually fused into the universality of modern science.' In this section of his work Dr. Needham discusses the originality of Chinese culture, the diffusion of ideas, the continuity of Chinese with western civilization (though here, following Janse, his criteria are perhaps insufficiently strict: it is surely necessary to distinguish between general likenesses and identities?), records, both western and Chinese, of contacts and indirect knowledge, routes, and specific cultural contacts between China and India, and China and the Arab world. In his discussion of maritime routes Dr. Needham is perhaps inclined to credit to India shipping which was in fact *k'un-lun*, for there is very little evidence for early Indian shipping. Nor does he mention the evidence for Korean craft in the Far Eastern trading system. Again the finds from South-East Asia of Far Western origin do not serve to identify the bottoms in which they were transported. But criticism must be restrained until the individual topics become available in the subsequent volumes: all who are interested in the inter-relations of cultures will find matter here worthy of study. It is to be hoped that it will be widely read, not only by professional students of its matter, but by all those who wish to understand the wider aspects of China and of its civilization. Dr. Needham is going far towards the achievement of Hooke's ideal of the perfecting of the discovery of 'an Empire of Learning, hitherto only fabulously described.' Indeed Dr. Needham's contribution is worthy of the old traditions of the Royal Society when it took all learning for its field, for here is to be found the blend of Humanity and Science which so distinguished that eminent body in the past. To his erudition the Cambridge University Press has added a skill and elegance indeed worthy of a country where scholarship and books have long been held in proper honour.

ANTHONY CHRISTIE

**The Great Loocchoo: A Study of Okinawan Village Life.** By Clarence J. Glacken. U. of Calif. P. (U.K. agents, C.U.P.), 1955. Pp. xvi, 324, maps and illustrations. Price £1 14s.

**205** This volume gives a detailed account of the life of the inhabitants of the Ryukyu Islands between Formosa and Japan as observed in three differing villages of Okinawa Island, whose family composition in 1951 is analysed and summarized in a brief appendix. The author deals successively with the geography and cultural history of the islands and of the three typical villages chosen for comparative study, with the dwelling, with village trade and finance, with the family system, inheritance, and the use and tenure of land; he describes the agriculture of Hanashiro and the other villages, the fishing industry and quarrying of Minatogawa and the forest industry of Matsuda; he gives a detailed account of the life of the individual in his or her family from birth to death including clothing, hair styles, crafts, medicine, etc., burial and family life. Further chapters deal with the life and organization of the community, with religion, and with environmental and social change. There are eleven pages of notes chapterwise giving detailed references to authorities and so forth; there are some 16 pages of good photographs and seven maps and plans.

Professor Glacken clearly appreciates the value of comparative and historical approaches to his material. The Ryukyus have been influenced in turn by Chinese and Japanese civilizations in the past and recently by American contacts. The culture of their inhabitants, though in many respects primitive, is varied occasionally by highly sophisticated traits. Rice is cultivated in irrigated terraces mostly without the aid of animal labour, and the most important of the domestic animals is the pig. It is only at the present day that pig-pens have ceased to be used generally (as they are, for instance, by the Konyak Nagas) as latrines; the bride at a wedding now arrives in a lorry instead of in a palanquin or a horse-drawn vehicle; tattooing, habitually practised on girls until the Japanese annexation (1879) and also by fishermen, has almost disappeared, and the favourite hair-style among the younger women is nowadays a permanent wave. In spite of the comparatively recent introduction of a cash

economy, there is a highly organized and somewhat complicated system of mutual loans, with interest, discount, and guarantors, operated among villagers, including groups consisting exclusively of women. A number of organizations exist for communal service or for the betterment of living conditions, but these are stated to have been introduced from Japan, though some of them—e.g. fire-brigades and road-cleaning gangs—are strongly suggestive of primitive communities throughout south-eastern Asia.

The author writes, generally speaking, as a human geographer rather than as an ethnographer, and his data are sometimes incomplete, as when he describes a corpse as being buried with the head

towards the west, the hands crossed on the chest and the knees drawn up to the chin, but omits to say whether the body is on its back or its side. At the same time he is a very sane observer who appreciates the fact that traits of behaviour may have serious significance for one section of a community while they have passed into customary formalities in another section of it and are thus launched on their way to become mere survivals—superstitions in the real sense of the word. Professor Glacken is to be congratulated on a careful and valuable contribution to our understanding of and acquaintance with a very little-known part of the world.

J. H. HUTTON

## CORRESPONDENCE

**Ritual Blowing.** Cf. MAN, 1956, 48, 148

**206** Sm.—Under the above heading Dr. Audrey J. Butt has written a very interesting paper on the art of healing or doing mischief among the Akawaio Indians of British Guiana. In conclusion she says: 'In spite of comparable data from other societies it does not seem practical to make a detailed comparison between Akawaio ritual blowing and any other system of magic spells or religious beliefs outside the South American area.' In this connexion I would like to say that ritual blowing is well known in Thailand (Siam) and in Laos. I speak from personal experience. In the month of June, 1910, I was lying severely wounded after a fight with a gang of rowdies (somewhere in north-eastern Siam) when a lance-corporal of mine came along and started blowing on my wounds while muttering certain magic incantations. Though I was subsequently patched up by a skilful medical man it was said that not he but the lance-corporal with his ritual blowing was responsible for my recovery! Quite a number of years after, during a long ride of inspection (also in north-eastern Siam), I was told about a certain witch or shamaness who understood the art of magic blowing, both for good and for evil purposes. Two Lao men living in different villages, separated by many kilometres, were bitter enemies and thus out for doing harm to each other. One of them therefore approached a renowned witch, and asked her, against payment, to do away with or harm his enemy in the other village. And one day, when his opponent was strolling peacefully in his own village, he was suddenly attacked by an unseen foe who left him bleeding on the ground. The sorely wounded man declared that the invisible attacker had behaved like a mad bull. It was a bull, a spirit bull, sent by the said witch, so declared the villagers unanimously, and even my own sergeant! Was the superstition of ritual blowing diffused from Asia to British Guiana, or is it a case of independent origin? I suppose that the latter explanation will be preferred by most people.

ERIK SEIDENFADEN

*Sørgensfri per Virum, Denmark*

**Radiocarbon Dates.** Cf. MAN, 1956, 147

**207** Sir.—Mr. Greenaway's suggestion for recording radiocarbon dates in such a way as to avoid any future ambiguity is obviously most desirable. As a matter of fact an identical proposal was made on identical grounds by Dr. Lee Abel of the Museum of Northern Arizona in 1953 (see *American Antiquity*, Vol. XIX, Part 2, p. 158).

However, in the interests of uniformity as well as economy of space and mental energy, would it not be even better to record the dates in terms of B.C. and A.D.? Surely the 'Christian' calendrical convention, although, as Dr. Abel points out, it is not universally recognized, must be sufficiently familiar to archaeologists the world over; and after all it is the dating style adopted for all other purposes by Western archaeologists.

*Harpden, Herts.*

H. J. BRAUNHOLTZ

*Note*

It may be thought that the present curious system ought to be modified before it becomes a stumbling block, though only a minor one, in the way of scientific advance. The dating, almost universally used and understood, from the birth of Christ (considered for this purpose as an arbitrary fixed point in time) surely has decisive advantages over a variable datum line, and it is perhaps a little strange that an idiosyncratic system so conducive to error (especially

in quotation at second and third hand) should have emanated from one of the exact sciences. In the original publication of the results of a test, there may well be a case for stating the estimated years of age at the time; but thereafter translation into the accepted time scale would seem desirable, as Mr. Brauholtz urges. Yet test results should never, surely, be quoted without giving also not only the date (year) of the test but also where or by whom it was carried out—if only because techniques are variable and are progressively refined. Contributors to MAN are therefore asked always to furnish this information when quoting results.

There is no intention of imposing any new system on contributors and readers, at least without wide consultation; but it is thought that as an interim measure it may be helpful if, where a contributor gives only a B.P. date (with date of test), the actual (B.C. or A.D.) date is added in brackets, with its margin of error. Unless therefore some better expedient emerges in the course of this correspondence, the Hon. Editor proposes to print such references in the following manner: '5961 ± 100 (1957, Utopia) [4004 B.C. ± 100.—ED.]'. References may, of course, be given in the simpler form '4004 B.C. ± 100 (1957, Utopia).' This is perhaps primarily a matter for the users rather than the originators of carbon-14 dates, but opinions will be welcomed from all quarters.—ED.

**Tribes and Castes of West Bengal.** Cf. MAN, 1956, 142

**208** Sir.—Your reviewer does somewhat less than justice, I think, to Mr. Mitra's *Tribes and Castes of West Bengal*, but what in any case does he mean when he describes some of the excellent material collected there as 'merely interesting' in contradistinction to 'worth having' and 'worthless'?  
*New Radnor*

J. H. HUTTON

'Utrilateral' and 'Utrolocal.' Cf. MAN, 1956, 30, 93, 170

**209** Sir.—I think that I have really said all that I want to about the term 'utrolateral.' I coined the word because, in my judgement, the Iban system of filiation is of a kind that does merit a clear and distinct name.

May I refer those interested in scrutinizing the evidence on which I based this judgement to an essay of mine, entitled 'The Family System of the Iban of Borneo,' which is to appear in the inaugural issue of the *Cambridge Annals of Social Anthropology*?  
*The Australian National University, Canberra* J. D. FREEMAN

**The Neanderthal Skull from Ganovce: Corrections.** Cf. MAN, 1956, 155

**210** After the October issue had gone to press, the Hon. Editor received a corrected proof from Dr. Vlček, mentioning the variant 'Hánovce' for the findplace of the skull and correcting 'Popred' in the first paragraph to 'Poprad.' Also too late for inclusion was a communication from Dr. A. A. Moss of the British Museum (Natural History), through Dr. K. P. Oakley, advising that the translation of the last eight lines of the sixth paragraph appeared to be defective and should read: '... made volumetrically, by titration with CeCl<sub>3</sub> in the presence of murexide (Brunischolz and Michod, *Helv. Chim. Acta*, Vol. XXXVII, 1954, pp. 598-602). Phosphoric acid is determined as Mg<sub>2</sub>P<sub>2</sub>O<sub>7</sub>, complexone II and citric acid being used as masking reagents. Phosphorus is expressed as PO<sub>4</sub> and the index is calculated from the formula: gF:gPO<sub>4</sub>=index.' In the seventh paragraph, the figures should be quoted as 0.0892.



1956

# MAN

*A Monthly Record of Anthropological Science*



The Finding of Haemoglobin D Disease in a Sikh  
(with Plate A)

Lt. Col. G. W. G. Bird, I.A.M.C., and Dr. H. Lehmann

Unilineal Descent Groups in the New Guinea Highlands  
R. F. Salisbury

Obituary  
R. M. Dawkins

Proceedings of the Royal Anthropological Institute

Shorter Notes

III Session of the Pan African Congress on Prehistory  
International Folk Music Council : IX Annual Conference

Reviews

General : Asia : Europe

Correspondence



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Contributions are welcomed by the Hon. Editor on all branches of anthropology (including prehistoric archaeology) and on related and ancillary fields of knowledge. They may be in the form of articles, illustrated or not, of about 3000 words, shorter notes (including reports of original research, reviews of recent work, and anthropological news), or letters, normally of less than 1000 words, on any anthropological topic, whether arising from articles in MAN or not.

All contributions must be typed in double spacing on one side of the paper only, and notes, references and other matter to be printed in smaller type should preferably be typed in triple spacing, never in single spacing. Notes and references should conform to the normal style of MAN; volume and plate numbers are given in upper-case Roman figures.

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The Discovery of Multiple Rock Gongs in Nigeria  
(with Plate B and a text figure)

Bernard Fagg

Indians in Africa

Dr. L. Homburger

Kinship and Farming in West Cumberland

W. M. Williams

Shorter Notes

Mycenaean and Indo-European : Trade-Wind Beads

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VOL. LVI, Articles 35-47

MARCH, 1956

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(with Plate C)

*Dr. Marian W. Smith*

The Distribution of Hæmoglobin C in West Africa

(with three text figures and four tables)

*Dr. G. M. Edington and Dr. H. Lehmann*

The May Tabu on Roman Marriage and a Parallel

*Professor S. Davis*

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## Ritual Blowing

'Taling'—A Causation and Cure of Illness among the Akawaio  
*Dr. Audrey J. Butt*  
(with Plate D and two text figures)

## Social Factors Influencing Industrial Output

*Tom Lupton*

## Proceedings of the Royal Anthropological Institute

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The Culture of the East African Coast in the Seventeenth and Eighteenth Centuries in the  
Light of Recent Archaeological Discoveries

(with Plate E and five text figures)

*The Revd. Dr. Gervase Mathew*

Zande Clan Names

*Professor E. E. Evans-Pritchard*

Shorter Note

Ghost Marriages among the Singapore Chinese: A Further Note

(with a text figure)

*Mrs. M. Topley*

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VOL. LVI, Articles 77-94

JUNE, 1956

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## Ibo Warfare

(with Plate F and two text figures)

*Dr. M. D. W. Jeffreys*

## Erotic Ibibio Tone Riddles

*Donald C. Simmons*

## Obituary

**Frederic Wood Jones, F.R.S.: 1879-1954**

*Miss Jessie Dobson*

## Shorter Note

### Acorn-Leaching Basins

*Dr. R. F. G. Spier*

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VOL. LVI, Articles 95-105

JULY, 1956

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**A Life-size Terra-cotta Head from Nok**

(with Plate G)

*Bernard Fagg*

**Ritualized Personal Relations**

*Dr. S. N. Eisenstadt*

**Shorter Note**

**Examination of 146 South Indian Aborigines for Haemoglobin Variants**

*Dr. H. Lehmann and P. K. Sukumaran*

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### R.A.I. CHRISTMAS CARD, 1956

The announcement of this year's Christmas card is being made earlier than usual in order to allow plenty of time for the Institute's Fellows overseas to place their orders and receive their cards in good time to send them out to any part of the world. It is hoped that they can be despatched early in October, and this means that an estimate of the total numbers required must be made by 15 September. Fellows overseas are particularly asked to send their orders before that date, by air letter if necessary. A further announcement will be made in the next issue of MAN.

This year's subject is the reproduction, published in MAN for May this year, of an early seventeenth-century portrait of a Princess of Zanzibar, who in 1604 married Sir John Henderson, with her maid. The painting is in the National Galleries of Scotland, and was used to illustrate Father Gervase Mathew's article (1956, 61) on 'The Culture of the East African Coast in the Seventeenth and Eighteenth Centuries.' The reproduction will be printed, probably in sepia, on an ivory card about 8½ inches wide by 5½ inches deep, and the customary greetings will appear on p. 3.

The card will be less expensive to produce than last year's, and every effort will be made to keep the price down to 9 shillings a dozen (including envelopes) for the first three dozen and 8 shillings for each additional dozen. These prices would be reduced by 1s. 6d. in each case for overseas orders, since Purchase Tax is not chargeable on cards consigned overseas by the printers. Definite prices will be announced in the August issue.

Attention is drawn to the fact that stocks are still held of the 1954 and 1955 cards, and that these may be bought at reduced rates.

VOL. LVI, Articles 106-128

AUGUST, 1956

# MAN

*A Monthly Record of Anthropological Science*



V International Congress of Anthropological and Ethnological Sciences

GREETINGS  
from the Royal Anthropological Institute

The Cruck Truss  
(with Plate H and six text figures)  
*Lord Raglan*

The Interpretation of Pakot Visual Art  
*Dr. Harold K. Schneider*

Proceedings of the Royal Anthropological Institute  
The Work of the Ethnomusicology Committee

Shorter Note  
Zande Totems  
*Professor E. E. Evans-Pritchard*

Reviews  
General : Africa : America : Asia : Europe : Oceania

Correspondence



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The card will be sold to Fellows in the United Kingdom at 9 shillings a dozen for the first three dozen and 8 shillings for each additional dozen; and to Fellows overseas at 7s. 6d. and 6s. 6d. respectively. All orders should be received by 15 September.

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*A Monthly Record of Anthropological Science*



Beauty and Scarification amongst the Tiv  
(with Plate I and eight text figures)

*Dr. P. J. Bohannan*

A Note on Tangu Dreams

*K. O. L. Burridge*

Obituary

Sir Arthur Keith, F.R.S.: 1886-1955

*Professor H. J. Fleure, F.R.S.*

Shorter Note

Notes on the Eton of the Southern French Cameroons

*Dr. Philip Dark*

Reviews

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VOL. LVI, Articles 150-171

OCTOBER, 1956

# MAN

*A Monthly Record of Anthropological Science*



The Stone Alignments of Southern Hyderabad  
(with Plate J, two text figures and a Gazetteer of Sites)

*Dr. F. R. Allchin*

The Coming of the Aryans and the End of the Harappa Civilization  
(with seven text figures)

*Professor Dr. Robert Heine-Geldern*

#### Obituary

Clarence van Riet Lowe : 1894-1956

*Dr. B. D. Malan : Professor A. J. H. Goodwin*

#### Shorter Notes

ABO Blood Groups in Kerman, South Persia

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### R.A.I. CHRISTMAS CARD, 1956

Orders should now be placed (preferably on the form supplied to Fellows with the September issue of MAN) for this year's Christmas card, which is an excellent reproduction of the painting of a Princess of Zanzibar with her Negro maid, first published in the May issue of MAN (1956, 61). The card, which will be ready very shortly, is sold to Fellows in the United Kingdom at 9 shillings a dozen for the first three dozen and 8 shillings for each additional dozen; and to Fellows overseas at 7s. 6d. and 6s. 6d. respectively. A specimen card, price 1 shilling including postage, will be sent to any Fellow on request.

# MAN

*A Monthly Record of Anthropological Science*



Alfred Reginald Radcliffe-Brown, F.B.A. : 1881-1955

A Memoir

(with Plate K)

*Professor Meyer Fortes*

The Chronology of the Harappan Civilization and the Aryan Invasions  
Recent Archaeological Research

(with a table)

*Walter A. Fairservis, Jr.*

#### Shorter Notes

The Centenary of Neandertal Man : Part I

The First International Congress of Human Genetics, Copenhagen

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### R.A.I. CHRISTMAS CARD, 1956

Supplies of this year's Christmas card should be available by the time when this issue of MAN appears. The subject is the painting of an Arab Princess of Zanzibar with her Negro maid, which was published with Father Gervase Matthew's article in the May, 1956, issue of MAN. A short account of how she fell in love with the Scottish soldier Sir John Henderson of Fordell in the early seventeenth century and helped him to escape death after being captured in Zanzibar is included on the card. Fellows are urged to apply immediately for their cards. Prices for cards and envelopes, post free, are:

First three dozen: 9 shillings a dozen.

Each additional dozen: 8 shillings.

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A Fellow has placed at the disposal of the Institute for sale a complete set of the JOURNAL OF THE ROYAL ANTHROPOLOGICAL INSTITUTE, New Series, from 1898 to 1954 (i.e. Vols. XXVIII-LXXXIV). For many years it has been impossible for the Institute to supply the complete series as several volumes or parts have long been out of print and are unobtainable. Such long and complete runs are worth much more than their face value of £2 a volume, but in this case the Institute has been enabled to offer the 57 (unbound) volumes for £125. Enquiries will be welcomed.

VOL. LVI, Articles 192-210

DECEMBER, 1956

# MAN

*A Monthly Record of Anthropological Science*



Human Figures in Wood and Ivory from Western Polynesia

(with Plates L and M and a text figure)

T. T. Barrow

A Note on Mancala Games in Northern Rhodesia

(with a text figure)

J. H. Chaplin

## Shorter Notes

A Bushongo Cup in the Musée de l'Homme: The Centenary of Neandertal Man, Part II

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*Hon. Editor of MAN: W. B. Fagg, M.A.*      *Hon. Assistant Editor of MAN: Miss M. A. Bennet-Clark, B.A.*

## R.A.I. CHRISTMAS CARD, 1956

All orders received have been executed, and sufficient supplies have been obtained to allow some further orders to be accepted, provided that they are placed with the Institute in the next few days. Cards will be despatched at once until supplies run out. They may be ordered only by Fellows, Associated Members and Affiliated Institutions, and any profits made will benefit the Institute's funds.

The subject of this year's card—which, as usual, bears no date—is the painting (belonging to the Earl of Buckinghamshire) of a beautiful Arab Princess of Zanzibar of the early seventeenth century who fell in love with the captive Scottish adventurer Sir John Henderson of Fordell and escaped with him to Egypt; a version of the story is printed on the card. The picture, which shows the Princess with her Negro maid, was reproduced in the May, 1956, issue of MAN, but any who wish may obtain a specimen copy by sending one shilling (postal order or U.K. stamps) to the Institute.

The price of the cards is 9s. a dozen for the first three dozen, 8s. for each additional dozen; or, in the case of orders despatched overseas by the printers, 7s. 6d. and 6s. 6d. Envelopes and ordinary postage are included.

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- Haiding, K. *Von der GebärdenSprache der Märchenerzähler*. Helsinki, 1955. 16 pp. (FF Comm. 155)
- Liungman, W. *Das wahrscheinliche Alter des Volksmärchens in Schweden*. Helsinki, 1955. 44 pp. (FF Comm. 156)
- Megas, G. *Der Bartlose im griechischen Märchen*. Helsinki, 1955. 16 pp. (FF Comm. 157)
- Paul, A., and F. Vopat. *Sl'uk . . . [Bratislava]*, 1955. 170 pp.
- Peuckert, W. E. *Lenore*. Helsinki, 1955. 19 pp. (FF Comm. 158)
- Popović, D. J. . . . *Les Serbes à Bačka jusqu'à la fin du dix-huitième siècle . . . [in Serbian]*. Belgrade, 1952. viii, 417 pp. (Posebna Izd. etnogr. Inst. Srpska Akad. Nauk. 3)
- *Les Serbes dans le Banat jusqu'à la fin du dix-huitième siècle [in Serbian]*. Belgrade, 1955. viii, 468 pp. (Posebna Izd. etnogr. Inst. Srpska Akad. Nauk. 6)
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The Council of the Royal Anthropological Institute announces that it will (subject as mentioned in the Rules for the time being governing the competition for the above prize) again award in 1957 the Curl Bequest Prize for the best essay by any competitor upon the results or analysis of all or any anthropological work carried out or published during the period of ten years preceding the year in which such essay is submitted and/or the history of some useful line in anthropology during that period.

Until further notice the rules governing the competition are :—

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- (2) They shall be in typescript in English, French or German, and three copies of each essay shall be submitted.
- (3) Essays shall be in literary form and not in the form of bibliographies or catalogues.
- (4) The length of an essay shall not exceed 25,000 words or be less than 10,000 words.
- (5) The decision of the Council of the Institute or of such officers of the Institute as the Council may from time to time appoint for the purpose of judging the respective scientific merits of the essays submitted shall be final as to the best essay and upon all other questions arising in connection with the essay competition.
- (6) If, in any year, there shall be no essay which, in the opinion of the Council of the Institute or of the officers of the Institute appointed for the purpose under the last preceding rule, is of sufficient scientific merit to deserve the award of the prize, then no award shall be made in that year. The amount of the prize available for that year shall be retained by the Institute and added to the prize in any later year in which there shall be at least two essays which are adjudged of sufficient scientific merit to deserve the award of the prize.
- (7) If in any year there shall be two or more essays which are judged of equal merit and scientific value and worthy of the award of the prize, then the amount available for the prize in that year may be divided.
- (8) The winning essay or essays shall be read at the last meeting of the Institute in December or at the first meeting in January of the following year.
- (9) The winning essay or essays shall be published in the *Journal of the Royal Anthropological Institute* or, at the discretion of the Council, may be published under its direction in the same style as the other publications of the Institute, or in both these modes.

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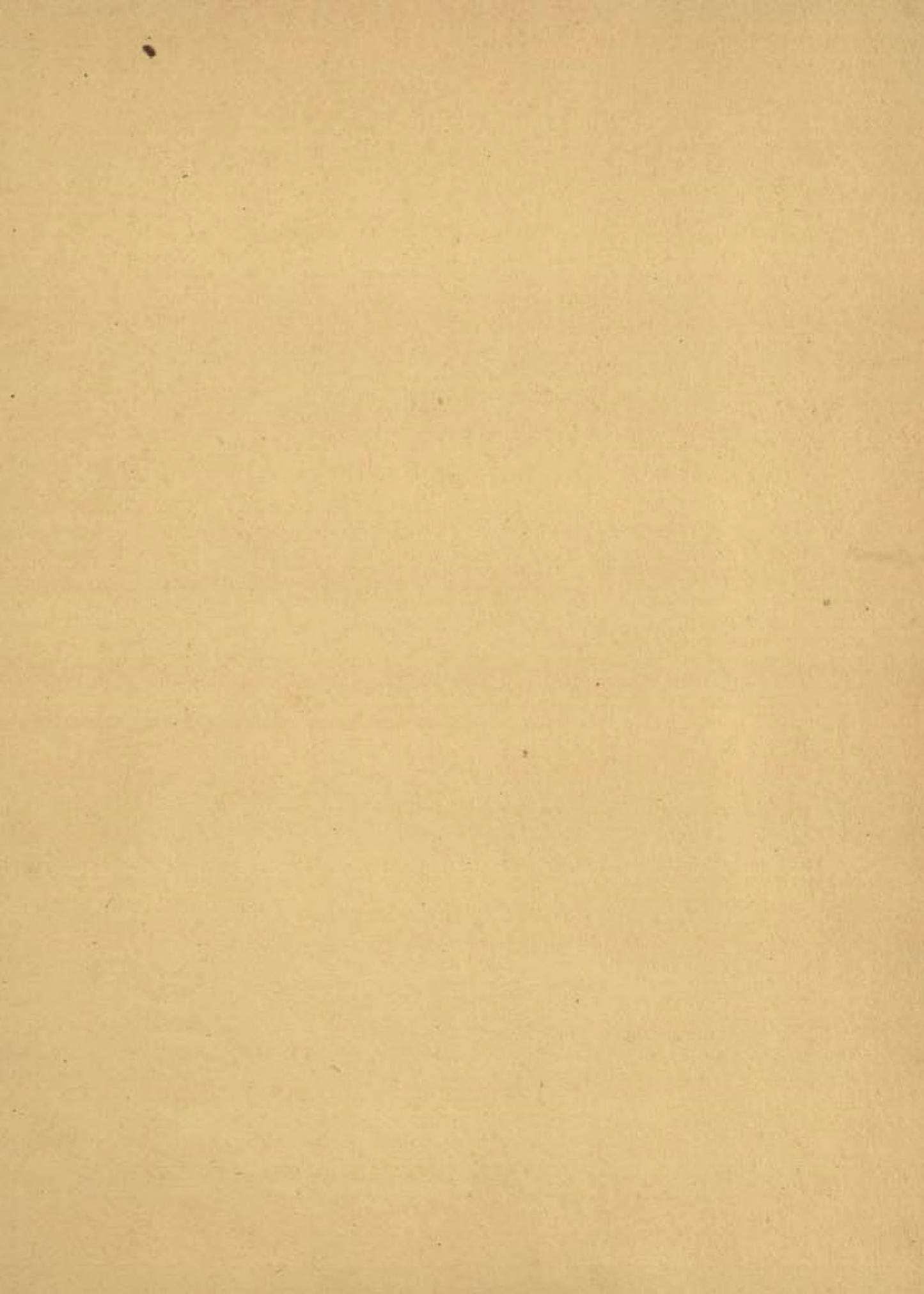
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